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THRESHOLDS FOR THE PERCEPTION OF DIFFERENCE IN FACIAL EXPRESSION AND ITS ELEMENTS

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No one can be so averse to analysis as to deny that there are degrees of generality among perceptions—for example, that one may perceive a tree and that he may also perceive the trunk of the tree, or a branch, or a leaf on that branch, or a vein in that leaf. In the same way, it is possible to perceive the expression of a face as a whole, or the peculiarities of the contours of the features—curve of lips, lines about the eyes, mouth, forehead, etc.—which are present at the same time. Now, one may say that a particular facial expression presupposes the existence of a certain disposition of these curves and lines upon the face, and no more assert that the expression *is the sum of them* than we should affirm that a tree is merely the sum of the trunk, branches, and leaves, or even of the cells into which we may analyze these parts under the microscope. We may call the expression the more general, and the curve of the lips or the creases at the corners of the eyes or the lines in the forehead the more particular, perception in this instance. But, if the perception of variations in expression be admitted to presuppose the *existence* of variations in these particulars, must it also presuppose the *perception* of them?

It is obvious that in the case, let us say, of a lump of metal the perception of the lump does not mean the perception of the indefinitely minute parts into which it can be divided; but this is not a correct analogy, since a smile is not composed of a large number of tiny smiles. Is it not possible, however, that a smile might be perceived

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when the changes in the curves and lines essential to it are imperceptible—reminding us of the visible grin of an invisible Cheshire cat? Common opinion seems ready to agree that such a thing not only is quite possible but happens continually,[1] that we often perceive things which can be analyzed into parts of a heterogeneous nature, parts which are not perceived singly; but of course our question is: *Can* they be perceived singly, whenever the more general perception is possible?

The experiment here reported deals with the difference thresholds of these two perceptions. The problem may be stated: to investigate the relationship between the thresholds for the perception of differences in facial expression and those for the perception of differences in certain elements of the expression. Sander in a review of the experimental results of the *Gestalt* psychology has indicated that the configuration in which a particular element is found may have an important bearing upon the difference threshold for this particular. He states that "die Unterschiedsschwellen abhängig sind von den jeweiligen Gestaltbedingungen, ... die Empfindlichkeit für Gestaltveränderungen im allgemeinen grösser ist als für Veränderungen isolierter Gegebenheiten." For example, "die Unterschiedsempfindlichkeit für Distanzveränderungen zweier Punkte ist erheblich gröber, wenn sie isoliert, als dann, wenn sie etwa als Pupillen eines Gesichtes dargeboten werden."[2] Similarly, Bühler reports that the difference thresholds for perception of the length of a line are higher than those for the perception of the difference between right angles in which this line forms the varying side.[3]

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[1]Cf. E. A. Singer, Jr., Studies in sensation and judgment, *Psychol. Rev.*, 4, 1897, 265. "It is well-known that where differences exist, comparatively slight in nature, between objects of considerable complexity of structure (such as human faces) one may be quite aware of a difference without being able to tell in what the difference consists." The conclusion of this experiment is that "the general judgment of difference has a lower threshold than the particular" (p. 271). The "general judgment" is defined as that judgment of difference "which cannot be explained as due to chance and to complete appreciation of the particular differences presented" (p. 270).

Cf. also Max Wertheimer's suggestion that a difference may be perceived without the ability to tell in what the difference consists. Untersuchungen zur Lehre von der Gestalt, *Psychol. Forsch.*, 4, 1923, 318.

Grace A. DeLaguna, Dualism and Gestalt psychology, *Psychol. Rev.*, 37, 1930, 200, says: "The little child . . . . . perceives and responds to a 'smiling face' before it has learned to see the curves of lip and cheek which compose it geometrically."

[2]F. Sander, Experimentelle Ergebnisse der Gestaltpsychologie, *Ber. u. d. X Kongr. f. exper. Psychol.* (Bonn), 1927, 46 f.

[3]K. Bühler, *Die Gestaltwahrnehmungen*, I, 1913.

So far as I have been able to ascertain, however, no one has experimented upon this relationship as it holds in the same figures with varying attitudes of the subject induced by different types of instructions.[4] It is toward this that the present experiment is directed. No attempt is here made to define "smiling," "frowning," or "neutral" expressions, but it will be shown that all the *S*s agreed in their choice of the direction of curvature which was toward smiling and that which was toward frowning, without any instruction upon the point. This agreement was sufficient for the purposes of the experiment.

Method

Apparatus. Five sets of cards, A, B, C, D, and E were used, each set consisting of 3 identical series of 7 cards. The cards were 91 mm. squares of stiff cardboard with a white unglazed surface.

Material A consisted of cards upon which were diagrammatic ('Leonardo') faces. Examples are given in Plate I. A circle 46 mm. in diam. was in the center of each card: the nose was represented by a straight line 7 mm. long with its lower end at the center of the circle; two horizontal lines (straight or curved) 10 mm. long and 10 mm. apart on a line 8 mm. vertically above the center of the circle represented the eyes; while a horizontal line (straight or curved), 10 mm. in length and 14 mm. vertically below the center of the circle, represented the mouth. All these lines were of the same breadth, approximately 'one point face' (0.35 mm.). The variations in the 7 different cards in each of the 3 series composing this set may be described as follows:

Card 1—eyes and mouth arcs of 3 cm. radius concave upwards;
Card 2—eyes and mouth arcs of 6 cm. radius concave upwards;
Card 3—eyes and mouth arcs of 12 cm. radius concave upwards;
Card 4—eyes and mouth straight lines;
Card 5—eyes and mouth arcs of 12 cm. radius concave downwards;
Card 6—eyes and mouth arcs of 6 cm. radius concave downwards; and
Card 7—eyes and mouth arcs of 3 cm. radius concave downwards.

Thus we have a series from greatest 'upward' curvature through the straight line to greatest 'downward' curvature. This material may be referred to as the *full face.*

Material B differed from Material A only in that here the mouth alone varied, the eyes remaining straight. This material may be called the *mouth face.*

Material C consisted of cards upon which there was but a single horizontal line, in the center of the card. This line was 10 mm. long and varied in curvature just as did the varying lines of Materials A and B.

Material D consisted of the cards of Material A, inverted.

Material E consisted of the cards of Material B, inverted.

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[4] R. K. White and Carney Landis have measured the least perceptible differences of variation in a silhouette, but seem not to have attacked the problem of the threshold for differences in facial expression. See The perception of silhouettes, this JOURNAL, 42, 1930, 431-435.

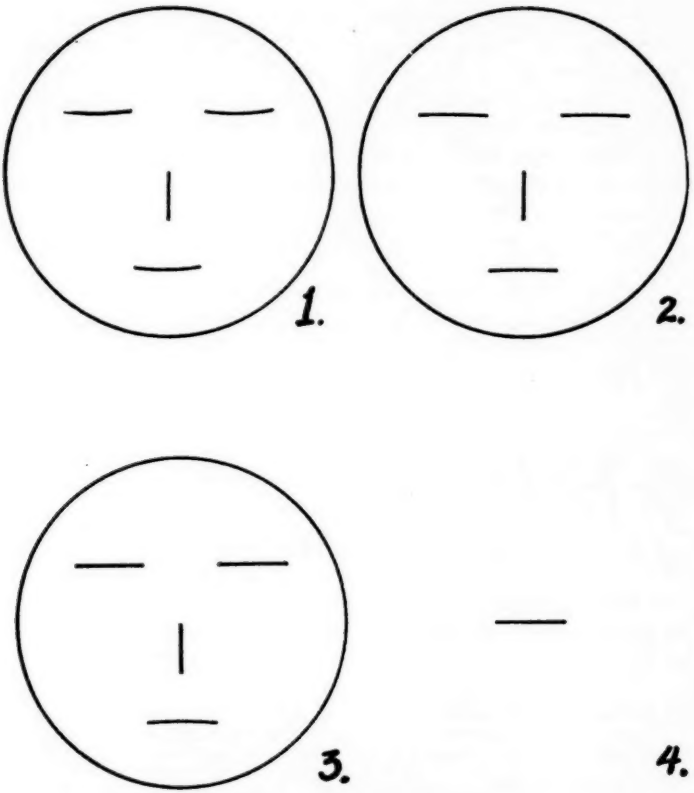


PLATE 1. MATERIALS

Key: 1 = Card 1, Material A; 2 = Card 5, Material A; 3 = Card 6, Material B; 4 = Card 4, Material C. The figures are reproduced actual size, but, when used, each figure was centered on a card 91 mm. square.

These figures were originally drawn full size by a professional draughtsman, and were then photo-engraved, with the use of a prism to prevent distortion, finally being printed upon the cardboard in black. The cutting was done by machine.

Lest dust or smudges of any kind lead to recognition of particular cards a large number were at hand, and sets were often replaced. Several times a *S* reported the belief that he could recognize a particular card; he was then told to report when next he saw the card, but in no instance was he successful in doing this.

The cards were exposed for a period of 0.18 sec. by means of a Whipple tachistoscope.[5] Care was taken to keep constant the lighting of the instrument.

The experiment was carried on from December 17, 1929, to May 21, 1930. Until February 3 the photographic dark-room of the Laboratory of Psychology at the University of Pennsylvania was used; it was then necessary to move to a room in the Hare Laboratory of Chemistry where lighting conditions were considerably different. However, no variations in the results that could be attributed to this change were noticed.

Procedure. The instructions used are presented below. It will be noted that there are two general types, (I) for 'curvature' and (II) for 'expression.' Type I necessarily varied slightly according to the material employed. Type II was the same for each material.

Instructions IA. You will be shown a series of diagrams of faces, each diagram being given but a short exposure. In some of the diagrams the eyes and mouth will be arcs turned upwards at the ends, in others, arcs turned downwards, and in others they will be straight lines. You are to make an immediate judgment for each diagram, reporting the eyes and mouth to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each diagram and to state it immediately. I shall say 'ready' a short time before the exposure of each face, and 'now' just as it is about to appear.

Instructions IB. You will be shown a series of diagrams of faces, each face being given but a short exposure. In some of the diagrams the mouth will be an arc turned upwards at the ends,[6] in others an arc turned downwards, and in others it will be a straight line. You are to make an immediate judgment for each diagram, reporting the mouth to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each diagram and to state it immediately. I shall say 'ready' a short time before the exposure of each face, and 'now' just as it is about to appear.

Instructions IC. You will be shown a series of lines, each line being given but a short exposure. Some of the lines will be arcs turned upwards at the ends, others, arcs turned downwards, and others will be straight lines. You are to make an immediate judgment for each line, reporting it to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each and line to report it immediately. I shall say 'ready' a short time before the exposure of each line, and 'now' just as it is about to appear.

Instructions ID. You will be shown a series of figures, each figure being given but a short exposure. Included in each figure will be three horizontal lines, some of which will be arcs turned upwards at the ends, others, arcs turned downwards, and others will be straight lines. You are to make an immediate judgment for each figure, reporting the lines to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each figure and to report it immediately.

It may be remarked that calling the inverted face a "figure," as in the preceding and following instructions, did not at all succeed in abolishing the idea of "face." Each of the 5 *Ss* thought it a good joke as soon as the "figure" was shown.

Instructions IE. You will be shown a series of figures, each figure being given but a short exposure. Included in each figure will be three horizontal lines. The

[5]Described in G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 264.

[6]"Upwards at the ends," etc., seemed the simplest way of indicating the direction of curvature of the arcs, a rather awkward thing to make clear without danger of confusion. The *Ss* seemed to understand perfectly what was meant.

lower two will always be straight, but the upper one, in which you are interested, will sometimes be an arc turned upwards at the ends, sometimes an arc turned downwards, and sometimes a straight line. You are to make an immediate judgment on each figure, reporting this line to be turned upwards, turned downwards, or straight, by saying 'up,' 'down,' or 'straight.' Be sure to make a judgment on each figure and to report it immediately.

Instructions II. You will be shown a series of diagrams of faces, each face being given but a short exposure. For each face you are to make an immediate judgment, as to whether it is smiling, frowning, or neutral. Be sure to make a judgment on each diagram and to state it immediately, remembering to judge only as to whether the face is smiling, frowning, or neutral, using the terms 'smile,' 'frown,' or 'neutral' in your report. I shall say 'ready' a short time before the exposure of each face, and 'now' just as it is about to appear.

The appropriate instructions were read to the *S* at the start of each experimental period, the sentence concerning the 'ready—now' signals being omitted after several sittings.

The *S* sat at a constant distance from the tachistoscope; the front legs of the chair were 2 m. from the machine. The 'ready' signal was given about 2 sec., and the 'now' signal 1 sec., before the exposure of the card.

Two orders of presentation of the 21 cards in a set of material were employed. The first was used while obtaining the first two difference-thresholds, and thereafter the first and second were alternated for each DL, the alternation beginning with the second order. The orders were as follows (the numbers being those of the cards as given above in the description of the material). First: 3, 1, 4, 7, 6, 2, 5, 1, 3, 7, 2, 4, 6, 5, 2, 6, 4, 7, 3, 1, 5; Second: 5, 7, 3, 1, 4, 2, 6, 7, 2, 4, 5, 1, 3, 6, 1, 4, 6, 3, 7, 5, 2. The use of these two orders precluded the possibility of the learning of the order of presentation. No *S* was able to demonstrate his having learned even a part of a series, though several tried to do this at various times.

Eleven DL were obtained from each *S*. Each DL was calculated from 50 observations of each of the 7 different cards in a set of material, with the exception that in the following cases 100 judgments of each card were obtained: *A*—the first 4 DL; *C* and *H*—the first 2 DL; *Ot* and *R*—the first 3 DL.

Thus, the results include for the 5 *S*s a total of 24,150 judgments. From 217 to 231 judgments were obtained at each experimental session. No DL was completed in a single sitting, and the exceptions noted above required three. An intermission of from 5 to 10 min. was introduced when each session was about half completed, usually after 6 series of 21 judgments had been made. An experimental period, including the intermission, usually lasted about 50 min.

Subjects. The *S*s in this experiment were Mr. Leon Arons (*A*), Dr. Stanley F. Cooper (*C*), Dr. John F. Humes (*H*), Mr. Jay L. Otis (*Ot*), and Mr. Thomas W. Richards (*R*)[7]. With the exception of *A*, a graduate student in psychology, the *S*s were members of the Department of Psychology at the University of Pennsylvania. *A*, *C*, and *H* were already familiar with tachistoscopic procedure; *A* and *C* had served in the preliminary experiment carried on to determine the details of the method to be followed in the main experiment, while *C* and *H* had been

[7]The author desires to acknowledge his indebtedness to these friends for their patience, and to Dr. G. H. Clark and Messrs. W. L. VanBuskirk and F. R. Gillespie, who served in the preliminaries to this experiment; and in particular to Professor Samuel W. Fernberger for his constant direction and encouragement.

TABLE I

SHOWING, FOR EVERY *S*, THE DL AND MEASURES OF PRECISION FOR EVERY ONE OF THE VARIOUS EXPERIMENTAL CONDITIONS

S		1	2	3	4	5	6	7	8	9	10	11
	Condition	IA	IIB	IC	IIA	IB	ID	IIA	IIA	IE	IC	IIA
	DL	1.0	1.5	1.0	1.3	0.9	0.5	1.6	1.2	0.2	0.3	1.3
A	h_1	0.30	0.26	0.37	0.29	0.38	0.52	0.47	0.40	0.43	0.54	0.45
	h_2	0.25	0.24	0.37	0.26	0.35	0.51	0.42	0.41	0.42	0.55	0.38
	Condition	IIA	IB	IIA	IC	ID	IIB	IA	IIA	IB	IIA	IE
	DL	1.4	1.4	1.2	1.1	1.3	1.2	1.1	0.8	0.7	0.6	1.3
C	h_1	0.25	0.34	0.41	0.44	0.59	0.36	0.45	0.43	0.46	0.55	0.34
	h_2	0.30	0.35	0.41	0.48	0.55	0.39	0.54	0.41	0.47	0.71	0.45
	Condition	IB	IIA	ID	IC	IIB	IA	IIA	IE	IIA	ID	IIB
	DL	1.3	1.0	1.0	1.5	0.7	0.8	0.6	0.7	0.6	0.8	0.7
H	h_1	0.24	0.33	0.43	0.54	0.31	0.52	0.39	0.40	0.41	0.49	0.44
	h_2	0.22	0.35	0.45	0.66	0.39	0.52	0.43	0.34	0.42	0.56	0.47
	Condition	IC	IIB	IIA	ID	IB	IIA	IA	ID	IIA	IE	IA
	DL	1.0	0.7	0.4	0.4	0.8	0.6	0.5	0.7	0.7	0.8	0.6
Ot	h_1	0.37	0.36	0.62	0.87	0.45	0.64	0.62	0.91	0.94	0.51	0.69
	h_2	0.37	0.34	0.64	1.01	0.64	0.65	0.68	0.81	0.45	0.65	0.98
	Condition	IIB	IA	IIB	ID	IIA	IE	IC	IIA	IB	ID	IIA
	DL	2.7	2.2	3.3	2.3	2.6	2.6	2.4	2.3	2.6	2.6	2.3
R	h_1	0.30	0.31	0.37	0.52	0.60	0.45	1.03	0.79	0.51	1.87	0.67
	h_2	0.19	0.32	0.21	0.46	0.55	1.17	0.83	0.84	0.79	0.63	0.37

Ss in other experiments in which the tachistoscope was used. The *Ss* were examined by an oculist, who reported normal or normally corrected vision in each case. None of the *Ss* had any information as to the purpose of the experiment.

Results

The results obtained by this method were treated in accordance with the phi-gamma hypothesis. Eleven difference-thresholds were obtained for each *S*. These DL and their coefficients of precision h_1 and h_2 are incorporated in Table I. Table II gives the DL for each *S*

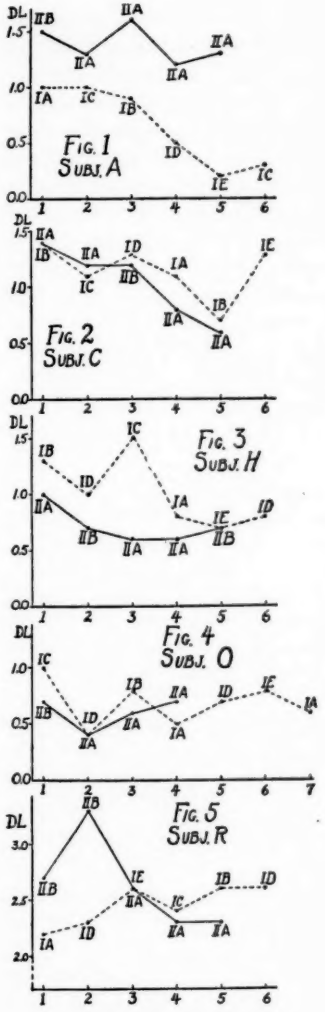
TABLE II

Showing, for Every *S*, the DL Arranged According to Their Experimental Conditions

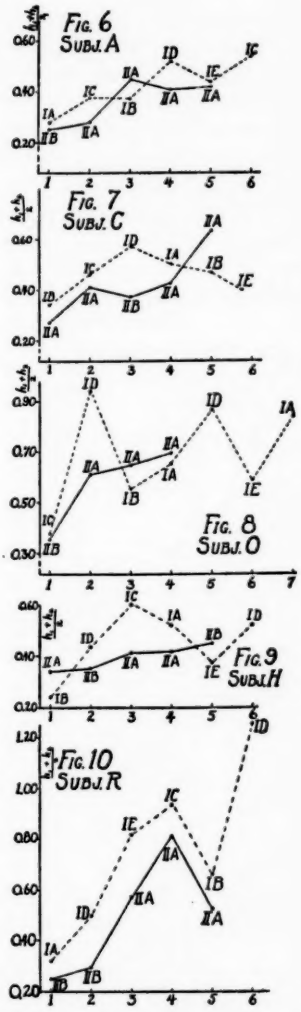
	Conditions						
S	IA	IIA	IB	IIB	IC	ID	IE
A	1.0	1.3	0.9	1.5	1.0	0.5	0.2
		1.6			0.3		
		1.2					
		1.3					
C	1.1	1.4	1.4	1.2	1.1	1.3	1.3
		1.2	0.7				
		0.8					
		0.6					
H	0.8	1.0	1.3	0.7	1.5	1.0	0.7
		0.6		0.7		0.8	
		0.6					
Ot	0.5	0.4	0.8	0.7	1.0	0.4	0.8
	0.6	0.6				0.7	
		0.7					
R	2.2	2.6	2.6	2.7	2.4	2.3	2.6
		2.3		3.3		2.6	
		2.3					

arranged as to the conditions (instructions and material) under which they were obtained.

In Figs. 1 to 5 are represented the DL for each *S*. The broken lines connect those for 'curvature' and the solid lines those for 'expression.' The ordinates represent the amount of the DL, and the abscissae give the order of succession of the various DL under each set of instructions. The notations at each point give the conditions



FIGS. 1-5. DL FOR EVERY *S*



FIGS. 6-10. COEFFICIENTS OF PRECISION FOR EVERY *S*

Broken line connects values for 'curvature;' solid line connects values for 'expression.'

under which that DL was obtained. Thus in Fig. 1 we find that *A*'s first 'expression' limen, obtained with Material B and Instructions II, amounted to 1.5, while his second 'expression' limen, with Material A and Instructions II, amounted to 1.3. Figs. 6 to 10 are a similar representation of the coefficients of precision, the average of h_1 and h_2 for each DL being graphed. In calculating the DL no absolute units of curvature were used. The stimulus intervals were regarded as equal to unity; hence the magnitudes of the DL and the h are in this sense arbitrary.

TABLE III

JUDGMENTS OF MATERIAL A UNDER CONDITIONS UNRESTRICTED AS TO TIME AND DISTANCE

	Subject				
	A	*C*	*H*	*Ot*	*R*
UP	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3
DOWN	5, 6, 7	5, 6, 7	5, 6, 7	5, 6, 7	5, 6, 7
STRAIGHT	4	4	4	4	4
SMILE	1, 2	1, 2, 3	1, 2, 3	1, 2, 3	1, "perhaps" 2
FROWN	7	5, 6, 7	5, 6, 7	5, 6, 7	7
NEUTRAL	3, 4, 5, 6	4	4	4	3, 4, 5, 6

In some cases the *S*s were asked to write introspections, although no suggestion as to their content or purpose was offered. Quotations from some of these reports will be presented in the discussion; and in view of their importance it is to be remembered that they were absolutely undirected.

At the conclusion of the experiment, a complete series of the 7 different cards of Material A was laid before each *S* individually, after the cards had been shuffled. *S* was then asked to judge each card as 'up,' 'down,' or 'straight,' an indefinite amount of time and as close observation as desired being permitted. When these judgments had been made, the cards were again shuffled, laid before the *S*, and judgments of 'smiling,' 'frowning,' or 'neutral' requested. The results of this experiment are recorded in Table III.

DISCUSSION

In the first place, it is obvious that under the conditions of the experiment the middle category *neutral* is essentially more elastic than the middle category *straight*. No line which is perceived as curved can rightly be judged as *straight*; yet it is quite in harmony with

the instructions, since no criterion of expression was imposed, to judge as *neutral* faces in which curvature of the lines was clearly perceived. The *neutral* category was also a much more convenient place to unload all doubtful judgments than the category *straight*. That at least 2 *S*s very probably took advantage of this elasticity is evident from Table III. *C*, *H*, and *Ot* under the conditions described judged as *smiling* each card perceived also as *up*, as *frowning* each card perceived as *down*, and as *neutral* the card perceived as *straight*. But though *A* and *R* were as accurate as the others in their judgment of curvature, they were not willing to attribute expression to all faces in which curvature was perceptible. Thus, *A* judged card 4 alone as *straight*, but cards 3, 4, 5, and 6 as *neutral*; while *R*, also judging card 4 alone as *straight*, judged cards 3, 4, 5, and 6 as *neutral* and card 2 as "perhaps" *smiling*.[8] The natural result of these tendencies, if carried into the main experiment, would be to make the DL for expression relatively greater than those for curvature in the case of these 2 *S*s. This presumption can be seen from Table II to be verified for *A*, whose DL were in general quite low; but it is doubtful as far as *R* is concerned—possibly because of the magnitude of his DL, which indicates his having had much difficulty in perceiving any differences at all except at the extremes of the stimulus series. From these considerations we may conclude that the experiment from its very conditions favored lower DL for curvature than for expression.[9]

'Learning,' as indicated by a downward tendency of the thresholds or by an upward tendency of the 'precision' might be due chiefly, first, to general adjustment of *S* to the conditions of the experiment; and, secondly, to the attainment of greater stability in his criteria of judgment. The first tendency might reasonably be expected to show itself in large measure only during the early part of the experiment, for 3 sittings of almost an hour apiece were required for each of the first 3 or 4 DL for each *S*. (Besides, *A* had observed in a period of 4

[8]It may be noted that there was complete agreement as to the extremes and the middle, card 1 being in all cases judged "smiling," card 7 "frowning," and card 4 "neutral." This relationship held also in the main experiment, the upper threshold always being that for "smiling" and the lower that for "frowning," while the point of subjective equality varied little from 4.

[9]Cf. S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 110 f.: "This is the real difference between the equality and the difference judgments. The difference judgments are much more 'objective' in nature. The equality judgments are more dependent on the attitude of the observer, on the form of instruction, on the observer's understanding of instructions and, as Angell pointed out, to an extent, at least, on his basic temperament."

sittings, and *C* in 6 sittings, during the preliminary work.) New types of material were introduced, however, at various times, so that the difficulty of establishing criteria should have been somewhat extended, perhaps over the whole course of experimentation.

As indicated in Figs. 1 to 5, these tendencies were variable. *A* shows a marked trend toward greater stability of criterion in the limens of curvature, and a much slighter one in the thresholds for expression. The advantage at both start and finish lies with curvature.

C shows a trend toward increased stability for expression which is emphatic; his series for curvature is more uncertain, though it is in general in the same direction except for a reversal in the case of the last DL. Here the advantage is with expression after equality at the start.

In the case of *H*, there is a more evident improvement in the criteria for curvature than in those of expression. The last 3 DL for each are practically constant.

The tendencies of *Ot* are indecisive. A sharp drop at the beginning is more or less neutralized thereafter.

R's criteria for expression show great improvement with the introduction of the *full face*, but his series for curvature has a general upward movement—the only case in the results which indicates decreasing precision of criteria.

In brief, then, in two cases, *A* and *H*, the criteria of curvature show a greater tendency toward precision than those of expression; for two other cases, *O* and *R*, the expression series shows more marked gain in stability than does the curvature series; *Ot*'s results show little improvement for either; and there is one case of a trend toward decreased stability, *R*'s curvature series.

It must already be evident that in spite of the large number of judgments upon which the DL are based, the variability is of such magnitude as to preclude statistical reliability for the differences among DL. The results must, then, be taken as suggestive rather than conclusive, certain tendencies being at least sufficiently marked to justify their being discussed.

The coefficients of precision (h_1 and h_2) in all cases show a general tendency toward improvement, as may be seen in Figs. 6-10. The curvature judgments have on the whole considerably less scatter for each *S* than have the expression judgments; but, on the other hand, for *C*, *H*, and *Ot* the precision increases more steadily for expression than for curvature, while for *A* and *R* the two increase at about the

same rate. In general, the precision for curvature, while greater than that for expression, is also more variable.

Since the variability is so great, it is difficult with 5 different types of material and only 11 DL for each *S* to determine the effect of these differences of material. The *full face*, whether upright or inverted, seems to give lower limens than the *mouth face* or the *single line*; but as between the two latter there appears little difference, excepting in the case of the *mouth face* with instructions for expression, which gave in general the highest DL. From Table II it can be seen that only for *H* did the *mouth face* with instructions for expression rank higher than fourth among the 7 different conditions, and that in two cases it ranked last. Evidently the *mouth face* is not as expressive as the *full face.*

One would likely suppose *a priori* that the thresholds would be lower when there were 3 lines by which to judge instead of one, and that it would be easier to judge the expression of a face with eyes and mouth varying than that of a face with the mouth alone varying. The fact that, in spite of the variability and of the small number of DL relative to the number of conditions, the indications are in line with such apparently reasonable expectations justifies confidence in the reliability of the results.

Perhaps the most interesting comparison is that concerning the effects of the two types of instructions, those for curvature (Instructions I) and those for expression (Instructions II). Fig. 1 shows *A*'s results. The successive DL under instructions to judge curvature are connected by the broken line, and those for expression by the solid line. It can be seen that *A*'s thresholds for curvature are considerably lower than those for expression, for there is not even an overlapping of the two curves. That *A* should have given such results is quite in accord with the fact that he tended to demand an easily perceptible curvature before he would judge the face as having any but a *neutral* expression. His introspections give some cues as to his criteria. After his first observation period, under conditions IA, he wrote:

I usually fixated the left eye. . . . . It was most infrequently that I saw the mouth. . . . . Occasionally both eyes appeared distinct—at times but one.

Under conditions IIB (his second threshold) he wrote:

I find myself using at least three criteria: (a) distance between eyes and mouth if great usually a frown,—if very short, a smile,—in between, guess frown or smile or neutral; (b) whether mouth was curved up or down: up—a smile, down—a frown; (c) length of mouth: if very short or curved (noticeably down), a frown; medium even when slightly curved, neutral; long, if curved up, a smile, if curved down, a frown. I used these (and others I don't remember) singly, together, etc., now one, now another.

The "medium, even when slightly curved, neutral" is significant, as is the evident difficulty he had in establishing a criterion of expression. No other light upon his criteria appears in his introspections.

Ot's results (Fig. 4) are not clear-cut, though what advantage there is appears to be in favor of expression. No introspective evidence as to his criteria is available.

It will be remembered that at the end of the experiment *R* showed, as did *A*, the tendency to demand much apparent curvature before judging the face as expressive. *R*'s thresholds (Fig. 5) are by far higher than any of the other *S*s', both for expression and for curvature; the upper and lower thresholds are near the limits of the stimulus series. His first two expression limens, with the *mouth face*, are considerably above the first two curvature limens, but, when the *full face* was introduced, the DL for expression dropped and fell below the succeeding curvature DL, though never quite reaching the low level of the first curvature DL. In an introspection upon the first DL (IIB) he stated:

Usually I fixate the mouth, often the eye-brows, but I try to see the thing as a whole.

On the second DL (IA) he wrote:

Since starting on the second series several things have occurred to me. I did not have a clear idea of the precise criterion for judging U, D, or S but it gradually became apparent. The first few times I judged them by the curvature of the single eyebrow, usually by the one on my left. Today I found that I was judging by looking at the eyebrow on my right, and at the right end. I tried to use the same method I had been using but could not seem to do so. Perhaps a limited distribution of attention prevents me from seeing the image as a whole and judging from that, for I cannot seem to do so in the short interval allowed.

C's results (Fig. 2) show a tendency in favor of keener sensitivity for expression than for curvature, after approximate equality at the start. Apparently enjoying the experimental session, he would often, when under instructions to judge expression, make such exclamations as, "Oh, what an ugly frown!" or "What a nice broad smile!" We have noted that *C* served as an *S* in the preliminary study when the procedure for this experiment was being worked out. At the first sitting of this preliminary work he was judging the curvature of a single line, and his introspection at the close of the period is of interest. He wrote:

Only a short space before the end of this experimental period I first associated the idea of *mouth* with a downward curve, and the thought 'sour-stomach' came. After that the idea of *mouth* connoting *face* came and the idea that there *should be* a face surrounding the oral groove. [*C*'s italics.] Several times thereafter a mild feeling of dissatisfaction was aroused by the fact that I could not *visualize* a facial setting. The 'mouth' association—as soon as it appeared—seemed to facilitate the perception of curves.

All this was written before he had seen or known anything about the face material. Later, after instructions to attempt to forget the *mouth* association he reported that it appeared very seldom. In the main experiment he gave the following introspections:

First DL (IIA): "At first I began using the eye-brows as my fixation point but after 4 or 5 exposures decided that mouth would be more satisfactory criterion—if 'up,' smile; 'down,' frown; 'level,' neutral. . . . . Principal criterion *mouth*."

Fifth DL (ID): "Noticed some confusion at first with the new type of material —'figures.' The Gestalt of '*face*' was so well established that the idea 'face upside down' came into consciousness immediately and persisted until the observation period was about three-quarters thru, when I became aware that 'face' was gone and I was observing simply two horizontal lines—curved or straight—on the lower part of the card. On the first two cards I tried to get my cue from the 'mouth;' finding this unsatisfactory I adopted the 'eyes' as my criteria and continued so until the end of the day's observation."

The most definite case of lower DL for expression than for curvature was that of *H*, as can be seen from Fig. 3. His introspections are correspondingly interesting.

First DL (IB): "Instead of making my judgments solely upon the direction of curvature, I frequently made them upon the basis of the 'general impression' which the individual stimulus left upon me. If the face looked pleased, it was 'Up;' if it looked displeased or as if it wore a frown, the judgment was 'Down.' If the face appeared to have neither of these expressions, it (the judgment) was 'Straight.' [Note that this was written without *S*'s knowing that he would later be asked to judge expression.] After the intermission I tried to hold myself more rigidly to the judgment of curvature alone, and during the remainder of the period I felt that I was making a greater number of 'Straight' judgments than before. [Not in accordance with the facts—the relative number of 'straight' judgments was almost exactly the same for the periods before and after the intermission.] The degree of subjective assurance was considerably less; and I felt that while fixating the 'mouth,' the other features were getting into consciousness by indirect vision, and thus influencing my judgment, somewhat as previously, by the method of 'general impression.'"

First DL (IB)—second sitting: "I tried to make all my judgments on the basis of curvature of mouth alone, but, as before, I felt that in many cases I was influenced by the 'general impression' which the diagram made. I found it difficult sometimes to fixate the mouth alone, as that frequently caused a blurring and I could report neither 'up,' 'down,' nor 'straight.' It seemed, in some exposures, that not only was the mouth curved, but the eyes as well. This circumstance is probably a function of the whole *Gestalt* (*i.e.* pattern) itself."

Second DL (IIA): "The change in *Aufgabe* today had the effect of modifying the *Einstellung*, in that a specific judgment as to curvature of the mouth was not required, but rather a judgment of *general impression*, in terms of 'smile,' 'frown,' or 'neutral.' As other features than the mouth may contribute to these characteristic expressions, the whole facial pattern was involved in my responses. The exposures are, of course, too short to permit of anything like analysis, and, as I can not, at least not often, re-image the diagram, the response must be made on the basis of something like 'snap' judgment. While I cannot be certain that other features besides the mouth are contributing to the 'general impression,' I sometimes believe they are. However, I am sure that the curvature of the mouth, when definitely perceived, is the deciding factor in many of my judgments."

Third DL (ID): "While the reversal of the figure minimized the apperceptive tendency to interpret it as a face, yet it was still a *face*; and the criterion, which has characterized the majority of my judgments, namely, a rather indefinite 'general impression,' seemed still to operate. To me, this belief was reinforced by the fact that I experienced considerable confusion at first in reporting the judgments. If the horizontal lines were turned down on the inverted figure, I had the impression of a 'smiling face,' which previously had been reported 'up,' or 'smile.' And on several occasions, these were my reports, which I immediately asked to have changed to 'down.'"

Since these introspections were written not as the result of specific questioning, but merely under instruction to write anything *S* deemed important (*H*, indeed, wrote several of his without being asked to do so), it seems significant that the two *S*s, *H* and *C*, for whom the DL for expression tended definitely to be lower than those for curvature, should have been just as definitely the only two for whom the effect of expression in the diagrams was very striking. Further, the fact that *any* of the *S*s should have given lower DL for

expression than for curvature is rather astonishing, when we remember our consideration of the *neutral* category as compared with the relative inelasticity of the *straight* category. Hence, while the inconclusiveness of the results from the statistical point of view must be admitted, the writer nevertheless believes it strongly suggested that the difference threshold for perception of facial expression in diagrams *can* be lower in the same individual than that for the perception of curvature.

From results as indecisive as those of the present experiment it would be extremely rash to draw inferences of a general nature; yet, because of the importance of the problem to the theory of perception, it is well to indicate briefly what would be implied in the outcome of such an experiment if it *were* possible to generalize. In the case that results (like *A*'s) were the rule, we should conclude that an attitude predisposing *S* to perceive the parts rather than the whole of a configuration favors the lowering of the differential limen, which very likely would be quite acceptable to the adherents of atomistic hypotheses. If, again, we found no valid differences to exist in the results (such as *Ot*'s), the descendants of Locke would be nothing dismayed. But how would this school explain lower thresholds for the perception of the configuration as a whole than for the perception of any part of that same configuration (toward which tend the results of *R*, *C*, and *H*)? What is present to assist the perception of the configuration, and not present to aid the perception of a part, unless the configuration itself?

Conclusions

(1) Thresholds for the perception of difference in facial expression and in the curvature of lines were obtained with diagrammatic material. A high degree of variability is present in the results.

(2) 'Practice effect'—the tendency toward progressive lowering of the DL—was present, though relatively slight.

(3) The 'precision' in general increased during the course of the experiment, and the increase was greater, though more variable, for curvature than for expression.

(4) The 'precision,' as a rule, was greater for curvature than for expression.

(5) The middle category of the expression judgments (*neutral*) was found to be more elastic than the middle category of the curvature judgments (*straight*).

(6) For one *S*, DL for curvature were consistently lower than those for expression. For another, the results were indecisive. For a third, the DL were lower at first for curvature but later for expression For two *S*s, the DL for expression were in general lower than those for curvature; and these two were shown by introspective evidence to have had strong tendencies toward perception of the face as a 'whole.'

(7) All *S*s agreed without instructions upon the direction of curvature present in the 'smile' and in the 'frown' respectively.

(8) The DL were lower, on the whole, for material in which three lines varied than for that in which but one line varied. This was particularly true in the DL for expression.

THE DISCRIMINATION OF SHORT INTERVALS OF TIME BY FINGER-TIP AND BY EAR

By Pearl Farwell Gridley, Northwestern University

The problem of comparing the various senses in respect to the discrimination of short intervals of time is an old one in the history of experimental psychology. Included in Czermak's program for experimentation in time perception, drawn up in 1857, is the suggestion that the shortest interval of time perceivable by the different senses be determined and that the senses be compared as to accuracy in judging the same time intervals.[1] This latter problem has never been systematically studied with any considerable number of subjects.

Most of the work in time perception has dealt with the sense of hearing alone. With one exception the early investigators were primarily or exclusively interested in this phase of the problem. Vierordt worked with vision and touch as well as hearing, but the data for each are scanty:[2] though some 2000 judgments are recorded they are spread over a large number of different time-intervals, with several different methods, dealing with the three different senses. All the early experimenters worked with few *S*s. Recent workers have used many *S*s but have confined their investigations to the sense of hearing.

The problem of comparing touch and hearing as regards time perception has acquired new interest in relation to Gault's work with the tactual discrimination of forms of speech.[3] He has demonstrated in a series of experiments with the teletactor that certain vowel, diphthongal, and consonantal qualities, as well as gross word and sentence patterns, may be distinguished through the finger tip.

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[1]H. Nichols, The psychology of time, this Journal, 3, 1891, 503.

[2]K. Vierordt, *Der Zeitsinn*, 1868. Reviewed by Nichols, *op. cit.*, 506-7.

[3]See especially R. H. Gault, 'Hearing' through the sense-organs of touch and vibration, *Jour. Franklin Institute*, 204, 1927, 353; A partial analysis of the effects of tactual-visual stimulation by spoken language, *ibid.*, 209, 1930, 437-458; On the effect of simultaneous tactual-visual stimulation in relation to the interpretation of speech, *Trans. Illinois State Acad. Sci.*, 22, 1930, 630-653.

A recent study from his laboratory has shown that *Ss* learn to distinguish sentences by the gross pattern[4]—pauses, accents, rhythm, and tempo—when no other criteria are present, and that when the gross patterns are the same, sentences can be distinguished, though less accurately, on the basis of vowels and consonants. It seems probable from the introspective reports of the *Ss* that discrimination on the basis of consonants is at least in part discrimination on the basis of time; some consonants give a "clipped-off" effect, others a "drawn-out" effect. Gross sentence pattern involves a multiplicity of factors, one of which is time. It is possible that Gault's results, in as far as they indicate an advantage accruing to the deaf interpreter of spoken language in a situation where he simultaneously sees the face of the speaker and feels his words, may be due in part to the ability of the deaf *S* to distinguish intervals by aid of his tactual sense. These considerations bring up the old problem of time perception. How small an interval of time can be distinguished by touch? How does touch compare with hearing in this respect? It was suggested that if a technique could be developed by which the vibrations from the Seashore phonograph record for the measurement of the "Sense of Time" might be transmitted to *S* through the receiver of the teletactor a comparison might be made between the two senses, a comparison on the basis of a test already widely used in the case of hearing; and that data could be accumulated concerning the threshold for the discrimination of short intervals of time by touch. Such is the purpose of the present investigation.[5]

Method

The problem required that *S* should make judgments concerning the relative length of short intervals of time laid off by auditory and by tactual stimuli. It was essential that the time-intervals be accurate and be the same for the two conditions of stimulation, touch and hearing, and that such factors as intensity, pitch, and duration of the stimuli be the same and, as nearly as possible, constant throughout the experiment. To meet these requirements it seemed best that the source of energy producing the stimuli, and its transmission to *S*, should be the same for touch and hearing.

Source of stimuli, their nature, and order. The Seashore phonograph record for the measurement of the "Sense of Time" was used to produce the stimuli. This is one of the series "Measures of Musical Talent," Columbia record 53002 D,

[4]S. F. R. Rose, The importance of pattern in learning sentences over the teletactor, unpublished Master's Thesis, Northwestern Univ., 1929.

[5]The author is indebted for the suggestion to Professor W. R. Roberts of Redlands University.

No. 3 A and No. 3 B. It was run on an electrically driven phonograph, and the vibrations were transmitted through an amplifier and telephone receiver and presented to *S* in an adjoining room as 'clicks' in the hearing condition, as 'impulses' against the finger-tip in the touch condition. Medium-tone steel needles were used. In the case of auditory stimulation *S* hears a series of clicks which come in groups of three. He is told: "You will hear three clicks marking off two intervals of time. If the second interval (that is, the time between the second and third clicks) is longer than the first interval, record L; if it is shorter, record S."[6] In the touch condition he feels these same stimuli as 'impulses' against the thumb.

The apparatus which produced the clicks from which the phonograph record was made is described briefly by Seashore[7] and more fully by Ross.[8] In the phonograph record *S* is given 100 trials, 20 for each of 5 increments. Each trial is a comparison between the standard 1-sec. interval and an interval which exceeds the standard by 0.02, 0.05, 0.09, 0.14, or 0.20 sec. In the series from which the record was made chance determined in each trial whether the standard or the longer interval should be sounded first. The key shows the order of presentation. The degree of difficulty, given in terms of 0.01 sec., is indicated at the head of each column. In the first block of 10 trials the increment 1.20 sec. is presented

Key[9]

	A	B	C	D	E	F	G	H	I	J
	20	20	14	14	9	2	2	5	5	9
(1)	L	L	L	S	L	L	S	L	S	S
(2)	S	S	S	L	S	L	L	S	L	L
(3)	L	L	S	S	S	S	S	L	L	L
(4)	S	L	L	S	L	L	L	L	S	S
(5)	S	L	L	L	L	L	S	S	S	S
(6)	S	S	S	L	S	S	S	L	L	S
(7)	L	L	S	L	S	S	L	S	S	L
(8)	L	L	L	S	S	L	S	S	L	S
(9)	S	S	S	L	L	S	L	L	L	L
(10)	S	S	L	L	L	L	L	L	L	L

for comparison with the standard (Col. A reading down); following a short pause, a second block of 10 trials is given with the same increment (Col. B). In the same way two blocks of 10 trials each are given with the increment 1.14, followed by one block of 10 trials with the increment 1.09. This completes the A-side of the disk (Cols. A to E). The B-side is similar, with two blocks of trials for increment 1.02, two blocks for increment 1.05, and one block for increment 1.09 (Cols. F to J).

To test the accuracy with which the phonograph reproduces the original sounds Seashore presents photographs of the path made by the prong of a vibrat-

[6]C. E. Seashore, *Manual of Instructions and Interpretations for Measures of Musical Talent*, 1919, 13.

[7]Seashore, *The Psychology of Musical Talent*, 1919, 108-9.

[8]F. B. Ross, The measurement of the time-sense as an element in the sense of rhythm, *Psychol. Monog.*, 16, 1914, 166-172.

[9]Seashore, *Manual of Instructions*, 9.

ing tuning fork upon the rotating phonograph disk. The test was made upon a spring type phonograph, wound by hand. The speed of rotation of the disk was found to be highly accurate; an error as large as 0.001 sec. in a single revolution was scarcely ever found.[10]

Transmission of stimuli. The phonograph used in the present experiment was a combination of two instruments. The original plan was to use a model No. 180 Columbia Viva-tonal Electric Radiograph, run by a synchronous motor. It is adjusted to run at the rate of 78 r.p.m., and the speed of the motor cannot be changed. The average phonograph record is best run at this speed. All the Seashore records except that for the sense of time are made for this standard rate. The time record, however, must be run at 60 to 65 r.p.m. This made the synchronous motor useless for our experiment. A Brunswick phonograph, Model No. T 160699, was substituted, run by a universal motor with speed regulator,

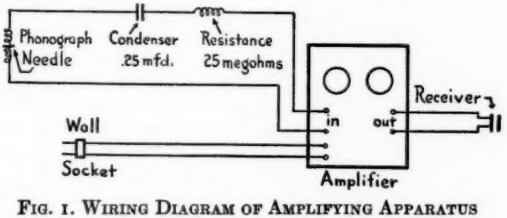


FIG. 1. WIRING DIAGRAM OF AMPLIFYING APPARATUS

driven by the city current, 110 A.C., 60 cycle. In combination with this the pick-up arm from the Columbia machine was used as described below.

The motor was set to run at 62 r.p.m. The constancy of its speed was tested by two methods: first by a Veeder Counter; and secondly by observing the number of times in a 1-min. period that a bit of paper, pasted on the record, passed a given point. Time was taken with a stop-watch and the full minute interval was used in each case. The second method was used as a check on the first, because of the possibility that excess pressure of the hand on the Veeder Counter would retard the speed. Readings were taken at both the beginning and the end of the record, that is, near the outer and near the inner limits of the disk. The mean of 26 readings was 61.5 r.p.m. with an S.D. of 0.31. This rough test was considered sufficient proof of the accuracy of the motor in view of the assurance given by Seashore's work concerning the high degree of accuracy of a good phonograph motor.

It had been hoped to take additional readings at the end of the experiment as a further check, but through a misunderstanding the mechanician in adjusting the apparatus changed the speed slightly. This occurred after the experiment was completed but before the second group of readings was taken. These readings, though useless as a check on the constancy of speed throughout the experiment, serve as an added check on constancy within a short period. Of the 30 readings taken at this time the mean was 63.21 revolutions per min. with an S.D. of 0.301.

[10]Seashore, *Psychology of Musical Talent*, frontispiece and 32-33.

A Western Electric two-tube amplifier, model 34-A, plugged in at a second socket to the wall current, was in circuit with the pick-up arm on the phonograph and a telephone receiver (cf. Fig. 1). The latter was in an adjoining room where *S* sat. In this way, vibrations from the phonograph needle in the grooves of the revolving record were picked up by means of the magnet in the pick-up arm and transmitted through the amplifier to the receiver. The amplifier may be set to give 4 successive degrees of amplification. In the description of the experimental procedure which follows these are referred to as amplifications 1, 2, 3, and 4, respectively. The receiver, which has a natural frequency of 800 d.v., is similar to an ordinary telephone receiver. It was made by the Bell Telephone Laboratory of New York, as a part of the Teletactor, for use by Professor Gault in the Vibro-Tactile Laboratory.

In circuit between the amplifier and the pick-up arm was a filter, consisting of a resistance coil of 25 megohms and a condenser of 0.25 mfd. This partially eliminated the vibrations caused by the scratch of the needle on the revolving record. These vibrations, scarcely audible to a listener with ear close to the needle, were very noticeable at the receiver end of the system when a high degree of amplification was used. Various types of condensers were tried in an effort to eliminate the scratch without reducing the energy of the clicks transmitted through the receiver, but with only partial success. The frequency of the scratch was so close to that of the clicks that it was difficult to cut out one without the other. The effect of inserting condensers in the circuit between the amplifier and the receiver was also tried, but with no better success. Aside from the scratch of the needle a second source of 'background' vibrations was found. When the switch in the current between the amplifier and the pick-up arm was shut off and the needle was not in contact with the record, vibrations from the revolving record were heard in the receiver. The probable explanation is an induced current produced at the point of contact of the electrical speed-regulator against the motor. Vibrations from these two sources, the scratch of the needle and rotation of the disk, were present throughout the experiment. They were faint when amplifications 1 and 2 were used, but quite pronounced with amplification 3. In the auditory condition they were heard as a background noise; in the tactual condition they were felt as a faint trembling of the diaphragm. In all cases, however, the auditory 'clicks' and tactual 'impulses' stood out clearly against this background.

Presentation of stimuli. (*a*) *Hearing condition.* In the hearing condition the receiver was suspended from a standard directly in front of the seated *S*. The height was adjusted so that it was on a level with *S*'s ears and about 3 ft. in front of him. *S* was seated in a chair with a writing arm.

(*b*) *Touch condition.* In the touch condition *S* held the receiver in his left hand, with thumb resting lightly on the diaphragm. He thrust his arm up to the elbow into a soundproof box.[11] This was of wood, 4 ft. square, completely filled with sawdust. Part way up the side was an opening large enough to accommodate *S*'s arm in a position comfortable to a seated person. A cloth sleeve attached to the opening was inserted into the box for a distance of 2 ft. Into this sleeve *S* thrust his arm, holding the receiver. A cloth was tucked about the arm

[11]The box is that constructed by Gault for use with the teletactor.

to fill any extra space and close the opening. The box in large measure deadened the sound from the receiver, but some *S*s could still faintly detect the clicks.

Earlier investigators in the laboratory have tried various means of controlling this factor.[12] The only one which proved successful was the use of a loud distracting noise. In the present experiment the same distraction was used. It consisted of two pairs of electrical buzzers, one pair suspended near the right ear, the other near the left ear of the *S*. The buzzers were all on one circuit and operated simultaneously. They produced a loud, continuous noise. Preliminary tests showed that, when the buzzers were operating, *S*, with arm and receiver thrust into the box with thumb removed from the diaphragm, was unable to detect the stimuli at all. The buzzers, while they proved successful in eliminating sound from the receiver, created another problem by providing a strong distraction in the touch condition which was not balanced in the hearing condition. The possibility of introducing a tactual distraction in the hearing condition was considered, but, since there seemed to be no possible means of equating two distractions presented to two different senses, it was not adopted. It was decided to recognize the distraction as a possible source of error in the touch condition and to make no effort to balance it in the hearing condition. A few of the *S*s reported that the distraction was definitely disturbing and made concentration difficult. A few reported that after the first few moments in the preliminary practice period they ceased to be conscious of the buzzer at all. Many reported that, while aware of it throughout, they were of the opinion that it did not affect their judgments.

Intensity of stimuli. The problem of controlling the intensity of the stimuli under the two conditions, touch and hearing, presented serious difficulties. Chief among these was the fact that in the touch condition this was partly under the control of *S*, whereas in the hearing condition it was not. The pressure of *S*'s thumb upon the diaphragm of the receiver dampened the vibrations. If considerable pressure was exerted, the distinct 'impulses,' corresponding to the 'clicks' of the hearing condition, were so deadened and blurred as to be indistinguishable from the faint 'background' vibration. In the preliminary practice period each *S* was warned of this fact and was instructed to allow his thumb to rest gently upon the diaphragm without exerting any pressure. *E* demonstrated the angle at which *S* was to hold the receiver so that the weight of the hand would not press the thumb against the diaphragm. *S* then thrust his arm with the receiver into the soundproof box and shifted it about slightly until a position was found where it could rest comfortably without tension and without pressure upon the diaphragm. In spite of these precautions a number of *S*s reported that in the effort to make accurate discriminations they unconsciously tensed the arm and pressed down upon the diaphragm. They were unaware of this reaction until they relaxed suddenly at the rest period or at the end of the sitting.

Even if *S* did not exert muscular pressure upon the diaphragm, the mere contact of the thumb against it dampened the vibration; that is, the amplitude and consequently the intensity of vibration was less when the thumb was in contact with the diaphragm than when the latter vibrated freely. A rough measure of this difference was obtained by comparing the loudness of the clicks when the

[12] M. A. Weichbrodt, Tactual and visual discrimination of consonantal endings, M.A. Thesis, Northwestern Univ., 1930, 15.

thumb was placed on the diaphragm (outside the soundproof box) with the loudness when the diaphragm was allowed to vibrate freely, as in the hearing condition. In preliminary work with four *Ss* it was found that the loudness produced by setting the amplifier at 1 in the hearing condition appeared to be approximately equal to that produced by setting the amplifier at 2 in the touch condition; *i.e.* contact of the thumb upon the diaphragm without muscular pressure reduced the loudness an amount equal to the change produced by reducing the amplification from 2 to 1 when the diaphragm was vibrating freely.

The four preliminary *Ss* agreed that with amplification 1 for hearing and 2 for touch the stimuli were always well above the threshold and easily sensed without strain. With a higher amplification for hearing an unpleasant roaring sound was produced; with a lower amplification for touch some of the stimuli were lost. This arrangement was, therefore, judged best, both because it appeared to approach optimal conditions for both touch and hearing and because it seemed the nearest approach obtainable to an equating of the physical intensity of the stimuli for the two conditions.

Further experimentation, however, showed that this rough measure of intensity was rendered totally unreliable by the fact, described above, that *S* might at any moment reduce the intensity of the stimuli even to a point where they fell below the threshold and were confused with the 'background' vibrations. To complicate the matter still further, some of the *Ss* complained that these degrees of amplification for hearing and touch respectively were not sufficient; that the stimuli were barely discernible and often fell below the threshold when the attention wavered. Other *Ss*, on the other hand, complained that when amplification was increased the stimuli became actually painful: in the hearing condition the sound was unpleasantly loud and harsh; in the touch condition the sensation was like an electric current running up the arm, distinctly painful.

In view of these difficulties it was decided to make no effort to present stimuli of equal intensity to all *Ss* under both conditions, but rather to allow each *S* to choose in the preliminary practice period that degree of amplification which appeared optimal to him for touch and hearing respectively. The majority chose amplification 1 for hearing and 2 for touch; some chose 1 for hearing and 3 for touch; a few chose 2 for hearing and 3 for touch. It was argued that, since the purpose of the investigation was to measure time perception and not sensory acuity, the best measure could be obtained by presenting each *S* with conditions optimal to him individually. This advantage coupled with the great difficulties of controlling intensity was the deciding factor in a choice of the method to be used.

In the preliminary practice period *S* was allowed to hear and to feel the different degrees of amplification and was told to choose that one in which the stimuli appeared most distinct and stood out most clearly from the background vibrations. He was warned concerning the effect of pressing upon the diaphragm with the thumb, and was told that if during the experiment the stimuli appeared to grow more faint he was to regulate the degree of contact by moving his thumb very slightly during a period between stimuli.

Subjects. Thirty *Ss* served in the experiment. Of these, 23 were undergraduate students in psychology at Northwestern University, 4 were graduate students, 1 was a professor of psychology, 1 a secretary, and 1 a social service worker.

Of the 30 only 3 had had previous experience with the teletactor. Two of these were graduate students (*Ss* 7 and 17) who had served in a preceding experiment on the reception of speech through the finger-tip: the third, a professor of psychology (*S* 13), had repeatedly served as subject in work with the teletactor. Of the 30 a number had at one time or another in their school experience been tested for musical ability by the Seashore records in the usual way.

Experimental procedure. Each *S* served for two sittings of approximately 40 min. each in April and May of 1930. The interval between sittings varied from 1 to 38 days. At each sitting, every *S* made 100 judgments by touch and 100 judgments by hearing; that is, both sides of the phonograph record were presented under both conditions. Thus, at the end of 2 sittings each *S* had made 200 judgments by touch and 200 by hearing. This gave for the group 6000 judgments by ear and 6000 through the finger-tip. To balance between the two conditions any practice effect within the period of experimentation, the ABBA order was used, as follows. The *Ss* were divided into 2 groups of 15 each. In the first sitting Group I made 100 judgments by touch, followed by 100 judgments by hearing, while Group II reversed this procedure, judging first by hearing, and then by touch. In the second sitting Group I started with hearing and Group II with touch.

At the beginning of the first sitting, preceding the main series, every *S* was given a preliminary practice period. Using the middle portion of the A-side of the phonograph record, *S* was allowed to hear the 'clicks' and feel the 'impulses' as he held the receiver in his hand. *E* counted with him "one, two, three" as the clicks occurred for two or three trials, and *S* made his judgments orally. The stimuli were continued until the nature of the task was thoroughly understood. The best method of holding the receiver was demonstrated. Then *S* thrust his hand with the receiver into the soundproof box, the buzzer was turned on, and he made a few judgments by touch. In cases where *S* seemed disturbed by the distraction he was allowed to sit for several minutes with the buzzer operating until he became more accustomed to the noise. As mentioned above he was allowed to hear and to feel the different degrees of amplification and choose which he preferred for each of the two conditions. The method of recording was explained, and attention was drawn to the longer interval occurring after every 10 trials. When the task was thoroughly understood and *S* at ease, the main series was begun. The time occupied by the preliminary practice was approximately 10 min.

During the main series *E*, engaged in the operation of the phonograph, was not in the same room with *S*. The A-side of the disk was run through without pause. Then *E* returned to the experimental room, glanced at the record sheet to make sure the task had been understood, and talked with *S* during a rest period of 2–3 min. During this rest period in the touch condition *S* withdrew his arm from the box to prevent numbness. The B-side of the disk was run through under the same condition. Following another rest period the second condition was presented in the same way. This too was broken by a rest period in the middle. The second sitting was like the first except that the preliminary instruction and practice were shorter, just sufficient to recall the procedure to *S*.

TABLE I

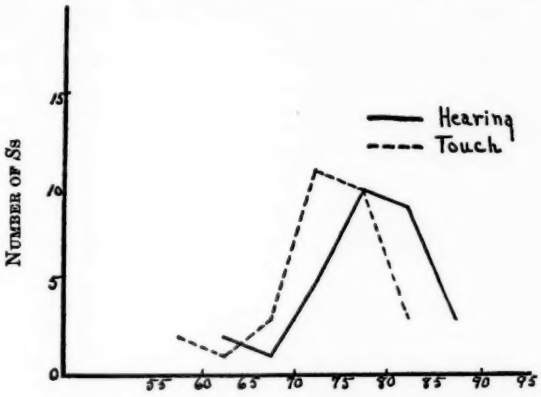
Record of Scores

	Hearing				Touch			
	A	B	C	D	E	F	G	H
S	Sit. I	Sit. II	Total	Av.	Sit. I	Sit. II	Total	Av.
1.	82	85	167	83.5	82	75	157	78.5
2.	71	75	146	73.0	68	78	146	73.0
3.	80	71	151	75.5	69	73	142	71.0
4.	85	90	175	87.5	75	85	160	80.0
5.	76	70	146	73.0	69	74	143	71.5
6.	78	85	163	81.5	66	74	140	70.0
7.	87	79	166	83.0	71	82	153	76.5
8.	69	75	144	72.0	75	79	154	77.0
9.	75	81	156	78.0	74	70	144	72.0
10.	66	57	123	61.5	59	60	119	59.5
11.	79	80	159	79.5	75	81	156	78.0
12.	80	73	153	76.5	74	78	152	76.0
13.	90	89	179	89.5	72	65	137	68.5
14.	80	81	161	80.5	72	78	150	75.0
15.	78	77	155	77.5	67	78	145	72.5
16.	75	79	154	77.0	60	66	126	63.0
17.	80	85	165	82.5	80	83	163	81.5
18.	85	81	166	83.0	79	83	162	81.0
19.	60	67	127	63.5	64	55	119	59.5
20.	77	74	151	75.5	65	71	136	68.0
21.	70	85	155	77.5	71	72	143	71.5
22.	78	86	164	82.0	78	80	158	79.0
23.	83	83	166	83.0	71	83	154	77.0
24.	75	74	149	74.5	69	71	140	70.0
25.	87	82	169	84.5	78	73	151	75.5
26.	63	74	137	68.5	66	76	142	71.0
27.	86	84	170	85.0	83	75	158	79.0
28.	76	81	157	78.5	70	74	144	72.0
29.	70	80	150	75.0	70	71	141	70.5
30.	70	75	145	72.5	69	66	135	67.5
	Sum = 2334.5				Sum = 2185.0			
	M_h = 77.82				M_t = 72.83			
	P.E. of M_h = .779				P.E. of M_t = .694			

Diff. between means = 77.82 − 72.83 = 4.99

$P.E._d$ = 1.04

M_t = 93.5% of M_h



Score on Test

Fig. 2. Distribution of Scores
Average of Sittings 1 and 2 using all increments

Results

Table I presents the scores of each *S* and the total group scores. The columns to the left give figures for the hearing condition; those to the right give figures for the touch condition. For each *S* the score is given for Sittings I and II, respectively, expressed in percent right or in number of correct judgments out of a possible 100; the total score for both sittings expressed in number of correct judgments out of a possible 200, and the average score of the two sittings. Since there were 100 trials per sitting, this last item may also be read as the total score expressed in percent right. At the bottom of the table the average or mean score for the group is given. The group mean for hearing, *i.e.* the average of the items in Column D, is indicated by the letter M_h; the group mean for touch, *i.e.* the average of the items in Column H, is indicated by the letter M_t. The P.E. of the group mean has been computed for hearing and touch respectively, and also for the difference between these means. The relationship between M_h and M_t is expressed both by number of correct judgments and by percentage.

Fig. 2 shows the distribution of scores for the group for both hearing and touch. The base line gives the scores, which for each *S* are the average scores of Sittings I and II, using all 5 increments. It is plotted from the items in Columns D and H of Table I.

TABLE II

RECORD OF SCORES FOR FIVE DEGREES OF DIFFICULTY

	Hearing										Touch									
	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T
Sitting	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II
Increment	20		14		9		5		2		20		14		9		5		2	
Subject																				
1.	20	19	18	20	16	18	17	18	11	10	19	20	20	19	20	12	15	16	8	8
2.	19	20	15	17	15	17	7	12	15	9	16	20	16	17	13	17	11	13	12	11
3.	19	20	16	16	17	15	14	7	14	13	17	17	17	19	10	13	13	14	12	10
4.	20	20	18	20	18	20	19	18	10	12	20	20	17	18	17	19	8	15	13	13
5.	20	19	14	15	16	14	12	11	14	11	16	16	15	18	16	13	13	13	9	14
6.	19	20	17	20	16	18	16	16	10	11	18	20	19	18	15	14	6	12	8	10
7.	20	20	20	19	18	17	15	14	14	9	18	18	15	19	17	17	10	15	11	13
8.	18	18	19	18	12	15	12	15	8	9	19	20	17	16	17	16	12	16	10	11
9.	20	19	18	18	12	16	14	18	11	10	18	19	15	15	14	16	16	11	11	9
10.	20	14	12	13	11	11	12	10	11	9	17	17	14	11	9	12	12	11	7	9
11.	20	20	19	20	17	16	13	13	10	11	17	20	19	18	18	19	11	11	10	13
12.	20	20	20	17	17	14	12	13	11	9	19	19	18	20	13	16	14	13	10	10
13.	20	20	19	19	20	19	14	13	17	18	20	18	16	14	15	14	12	11	9	8
14.	20	20	19	20	17	18	12	14	12	9	18	19	20	20	12	16	14	10	8	13
15.	17	19	16	19	16	14	13	13	16	12	18	18	17	19	11	13	10	16	11	12

TABLE II (Continued)

16.	19	20	18	19	14	15	13	13	11	12	10	13	13	16	11	14	14	12	12	11
17.	20	19	19	20	16	18	14	15	11	13	19	20	18	19	14	19	17	15	12	10
18.	19	20	16	17	19	16	17	14	14	14	19	19	17	17	17	16	17	18	9	13
19.	14	17	15	9	14	13	8	13	9	15	17	15	16	14	7	8	12	11	12	7
20.	20	20	17	20	16	14	14	10	10	10	17	16	16	17	8	17	12	10	12	11
21.	17	20	20	20	12	15	11	16	10	14	20	18	13	18	14	10	12	13	12	13
22.	20	20	19	20	18	19	10	14	11	13	19	20	19	20	17	18	14	13	9	9
23.	20	19	19	19	15	19	15	17	14	9	16	20	14	20	15	16	11	14	15	13
24.	20	17	18	18	11	15	12	15	14	9	15	18	18	15	14	15	13	11	9	12
25.	20	20	20	19	17	15	14	14	16	14	20	20	17	19	15	13	13	12	13	9
26.	16	19	14	15	8	13	14	17	11	10	11	18	14	19	17	15	11	13	13	11
27.	20	20	19	19	16	19	15	16	16	10	20	19	20	18	13	16	17	12	13	10
28.	16	20	16	19	16	16	17	15	11	11	16	20	18	17	15	15	13	10	8	12
29.	19	19	17	19	9	14	15	17	10	11	17	17	16	17	13	18	14	14	10	5
30.	18	18	15	18	12	12	12	12	13	15	15	12	17	14	12	12	14	16	11	12
Sum	570	576	522	542	451	475	403	423	365	342	521	546	501	521	419	449	381	391	319	322
Sum I & II	1146		1064		926		826		707		1067		1022		868		772		641	
Av.	38.20		35.47		30.87		27.53		23.57		35.57		34.07		28.93		25.73		21.37	
% rt.	95.50		88.68		77.12		68.83		58.93		88.93		85.18		72.33		64.33		53.43	

Table II presents the scores according to the various increments or degrees of difficulty, in the case of both hearing and touch. The figures directly below the letters designating the columns (A, B, C, etc.) give the sitting, either first or second. The row below this shows the increment in hundredths of a second, from the least degree of difficulty, that of twenty hundredths, to the greatest, that of two hundredths. The vertical column at the extreme left gives the numbers of the *S*s from 1 to 30.

The table should be read as follows: In the hearing condition *S* 1 in Sitting I out of 20 trials with increment 20, made 20 correct judgments, a perfect score (Column A). In Sitting II with the same increment she made 19 correct judgments (Column B). With increment 2, however, she made only 11 correct judgments out of 20 trials (Column I), and in Sitting II only 10 (Column J). This last is exactly what would be expected by chance, if all the judgments were mere guesses, and thus shows total lack of discrimination with this degree of difficulty. In the touch condition with increments 20 and 14 (Columns K, L, M, N) *S* 1 made as good a score as she did in the hearing condition. In the case of increment 9 Sitting II (Column P) is markedly lower than Sitting I (Column O), and the two together average somewhat lower than the score obtained with the same increment under the hearing condition. Increments 5 and 2 are also lower for touch than for hearing. With touch the record for increment 2 is even lower than that to be expected by pure chance.

The horizontal row directly below the record of *S* 30 gives the sum of the various columns, *i.e.* the number of correct judgments made by the group for the various increments in Sittings I and II. Below this is given the total number of correct judgments made by the group when the two sittings are combined for each of the several increments. Thus, in the hearing condition with increment 20 the group made 1146 correct judgments out of a possible 1200. Below this appear the averages. For example, in the hearing condition with increment 20 the *S*s on the average judged correctly in 38.2 of the 40 trials. The last row gives this fact in terms of percent right; 38.2 correct judgments is 95.5% right of the total number of trials given with increment 20.

Fig. 3 is plotted from the figures given in the bottom row of Table II, showing the average score in percent right for each increment for all *S*s. The abscissa gives the several increments in hundredths of a second, thus showing the different degrees of difficulty from the easiest at the extreme left to the hardest at the extreme right. The ordinate shows the score in percent right. The horizontal line indicates the conventional threshold of 75% correct judgments.

Table III shows the practice effect between Sittings I and II as shown by group scores. The score for the group, expressed in number of correct judgments, is given for each of the two sittings for the several increments. These figures are the same as those given in

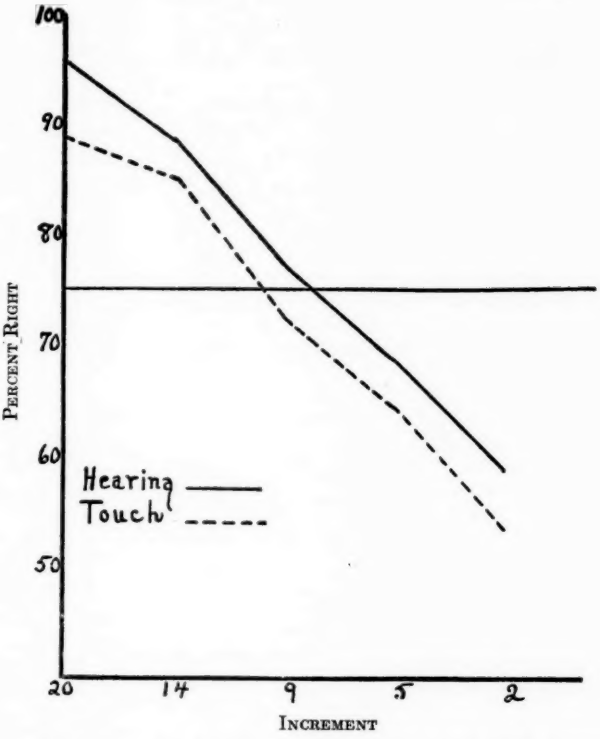


FIG. 3. AVERAGE SCORE OF PERCENT RIGHT AT EVERY INCREMENT

Table II, in the horizontal row labeled "Sum" directly below the record of *S* 30. The scores are also given in percent right, and the percent of increase of the second sitting over the first has been computed in each case. The total score, when all increments are considered together, expressed both in number of correct judgments and in percent right, is given for each sitting, and the percent of increase of the total score of the second sitting over the first. In the same way the last horizontal row gives the group score when only the first three increments are used, expressed in number of correct judgments and in percentage.

TABLE III

PRACTICE EFFECT BETWEEN SITTINGS I AND II SHOWN BY GROUP SCORES

	Hearing				
	Sitting I		Sitting II		Increase of
Increment	Score	% right	Score	% right	II over I
20	570	95 %	576	96 %	1.05%
14	522	87 %	542	90.3%	3.83%
9	451	75.2%	475	79.2%	5.32%
5	403	67.2%	423	70.5%	4.96%
2	365	60.8%	342	57 %	−6.30%
Total	2311	77.03%	2358	78.6%	2.03%
First 3	1543	85.72%	1593	88.5%	3.24%

	Touch				
	Sitting I		Sitting II		Increase of
Increment	Score	% right	Score	% right	II over I
20	521	86.8%	546	91 %	4.79%
14	501	83.5%	521	86.8%	3.99%
9	419	69.8%	449	74.8%	7.16%
5	381	63.5%	391	65.2%	2.62%
2	319	53.2%	322	53.7%	.94%
Total	2141	71.4%	2229	74.3%	4.11%
First 3	1441	80.05%	1516	84.22%	5.20%

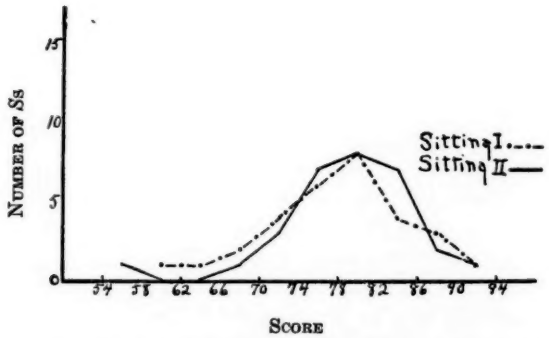


FIG. 4. DISTRIBUTION OF SCORES IN HEARING CONDITION FOR SITTINGS I AND II

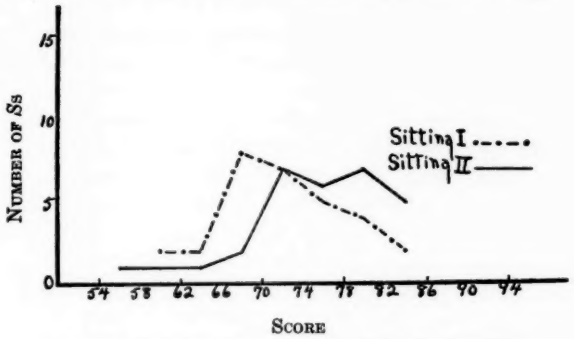


FIG. 5. DISTRIBUTION OF SCORES IN TOUCH CONDITION FOR SITTINGS I AND II

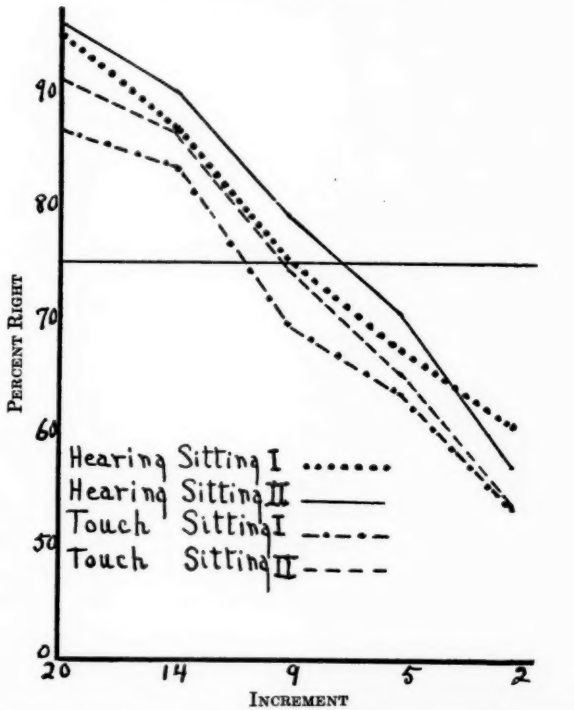


FIG. 6. AVERAGE SCORE OF PERCENT RIGHT AT EVERY INCREMENT FOR EACH SITTING AND FOR EACH CONDITION

Figs. 4 and 5 compare the distributions of scores in the two sittings. Fig. 4 gives for the hearing condition the frequency curves of Sitting I and Sitting II. The base-line gives the score in number of correct judgments out of a possible 100. The vertical axis gives the number of *S*s. The curves are plotted from the items in Columns A and B in Table I. Fig. 5 gives the corresponding data for the touch condition. It is plotted from the items in Columns E and F in Table I.

TABLE IV

FREQUENCY TABLE SHOWING THE DISTRIBUTION OF SCORES FOR THE SEVERAL DEGREES OF DIFFICULTY

		Sitting I					Sitting II				
	Increment	20	14	9	5	2	20	14	9	5	2
	Score										
Hearing	20	17	4	1			17	9	1		
	19	6	8	1	1		8	9	4		
	18	2	5	3			2	4	4	3	1
	17	2	3	5	3	1	2	3	2	3	
	16	2	4	8	1	3		1	4	3	
	15		3	2	4	1		2	6	4	2
	14	1	2	2	7	6	1		5	5	3
	13				3	1		1	2	6	3
	12		1	4	7	1			1	2	3
	11			2	1	9			1	1	5
	10				1	6				2	5
	9			1		1		1			8
	8			1	1	1					
	7				1					1	
Touch	20	5	3	1			11	4			
	19	6	3				5	7	3		
	18	5	4	1			6	6	2	1	
	17	6	7	6	3		3	5	3		
	16	4	5	1	1		2	2	7	4	
	15	2	3	5	1	1	1	2	3	3	
	14		3	4	6			3	3	3	1
	13		2	4	5	4	1		4	6	7
	12			2	6	7	1		3	4	4
	11	1		2	4	4		1		6	5
	10	1		1	2	4			1	3	5
	9			1		5					4
	8			1	1	4			1		2
	7			1		1					1
	6				1						
	5										1

Fig. 6 also shows the practice effect between sittings. It is plotted from the items given in Table III. The average score, in percent right, for the group is given, for each increment, for each of the two sittings, for hearing and for touch.

Table IV presents the data in the form of a frequency table, showing the distribution of scores for the several degrees of difficulty for each of the two sittings. The figures at the top of the vertical columns indicate the increments from 20 to 2. The column at the extreme left gives the score in number of correct judgments, from a perfect score of 20 at the top to the poorest score made, that of 5 in the case of touch, at the bottom. The horizontal line indicates the conventional threshold of 75% correct judgments, which in this case is 15 of the 20 trials. The table should be read as follows: In the hearing condition, Sitting I, with increment 20, 17 *S*s made a perfect score, *i.e.* out of the 20 trials given with that increment each of these *S*s made 20 correct judgments. In the touch condition, Sitting I, with the same increment, only 5 *S*s made a perfect score. In the hearing condition, Sitting I, with increment 2, 5 *S*s had scores above the threshold, while in the touch condition there was one.

Table V gives the amount of the practice effect between sittings in the case of each S, shown by increase in score-points of the second sitting over the first. When the score of the first sitting exceeds that of the second the difference in score-points is indicated by a negative number. Thus in the case of *S* 1 the second sitting exceeded the first by 3 points in the hearing condition, but in the touch condition the score of the second sitting was 7 points below that of the first.

Table V gives also a comparison between hearing and touch in terms of the number of score-points by which hearing exceeds touch. This is given in the case of each *S* for each sitting and for the average scores of the two sittings. In those cases in which touch is superior to hearing the difference in score-points is indicated by a negative number. Thus in the case of *S* 2 a comparison of the hearing and touch scores made at the first sitting shows that hearing exceeded touch by 3 score-points, but at the second sitting touch was superior to hearing by 3 score-points.

Fig. 7 shows by means of a scatter diagram the amount of correlation between hearing and touch. The scores used are the average of the two sittings, expressed in percent right.

TABLE V

PRACTICE EFFECT BETWEEN SITTINGS AND SUPERIORITY OF HEARING OVER TOUCH SHOWN IN SCORE-POINTS FOR EACH *S*.

S.	Points Increase Sit. II over I		Points Hearing Exceeds Touch		
	Hearing	Touch	Sit. I	Sit. II	Av.
1	3	− 7	0	10	5
2	4	10	3	− 3	0
3	− 9	4	11	− 2	4·5
4	5	10	10	5	7·5
5	− 6	5	7	− 4	1·5
6	7	8	12	11	11·5
7	− 8	11	16	− 3	6·5
8	6	4	− 6	− 4	− 5
9	6	− 4	1	11	6
10	− 9	1	7	− 3	2
11	1	6	4	− 1	1·5
12	− 7	4	6	− 5	·5
13	− 1	− 7	18	24	21
14	1	6	8	3	5·5
15	− 1	11	11	− 1	5
16	4	6	15	13	14
17	5	3	0	2	1
18	− 4	4	6	− 2	2
19	7	− 9	− 4	12	4
20	− 3	6	12	3	7·5
21	15	1	− 1	13	6
22	8	2	0	6	3
23	0	12	12	0	6
24	− 1	2	6	3	4·5
25	− 5	− 5	9	9	9
26	11	10	− 3	− 2	− 2·5
27	− 2	− 8	3	9	6
28	5	4	6	7	6·5
29	10	1	0	9	4·5
30	5	− 3	1	9	5

Summary:

Hearing

Sit. I exceeds Sit. II 12 *Ss.* " " " 56 points
Sit. II " Sit. I 17 *Ss.* " " " 103 points
Sit. I equals Sit. II 1 *S*

Touch

Sit. I exceeds Sit. II 7 *Ss* " " " 43 points
Sit. II " Sit. I 23 *Ss* " " " 131 points
Sit. I equals Sit. II 0 *Ss*

Summary:

Average scores

H exceeds T 27 *Ss.* " " " 164.5 score points
T " H 2 *Ss* " " " 7.5 score points
H equals T 1 *S*

Sitting I

H exceeds T 22 *Ss* " " " 184 score points
T " H 4 *Ss* " " " 14 score points
H equals T 4 *Ss*

Sitting II

H exceeds T 18 *Ss* " " " 159 score points
T " H 11 *Ss* " " " 30 score points
H equals T 1 *S*

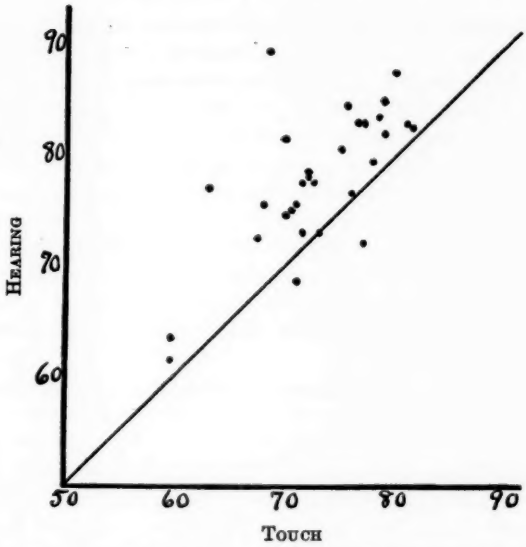


FIG. 7. SCATTERGRAM SHOWING AMOUNT OF CORRELATION BETWEEN TOUCH AND HEARING
(Average Scores of Percent Right, Sittings I and II)

DISCUSSION OF RESULTS

How does touch compare with hearing when average group scores are taken as the basis of comparison? When the average from the two sittings is used as the score for each *S* and group scores are computed on this basis, it is clear that the group showed a greater accuracy in the discrimination of short intervals of time by ear than in the discrimination of the same intervals of time through the finger tip. The group mean for hearing is 77.82 correct judgments out of a possible 100, with a P.E. of 0.779 (see Table I), while for touch the group mean is 72.83 ± 0.694. The fact that the difference between the means is slightly more than 4 times as great as its probable error, 4.99 ± 1.04, indicates that this difference is a significant one. Fig. 2 shows this superiority of hearing over touch when total scores are considered. Fig. 3 shows that when scores for the 5 increments or 5 degrees of difficulty are considered separately hearing exceeds touch in every instance. This superiority varies from 3.5% in the case of increment 14 to 6.57% in the case of increment 20 (Table II).

When scores for the two sittings are considered separately, practice effect is found to be an important factor, a factor which raises doubt as to the validity of the tentative conclusion concerning the superiority of hearing over touch. Table III and Figs. 4, 5, and 6 reveal the increase of score of the second sitting over the first. For both hearing and touch the group scores for Sitting II are higher than those for Sitting I, not only when total scores are considered, but also in the case of the scores for each of the 5 increments with one exception. The exception is found in the hearing condition with increment 2, where the first sitting exceeds the second (Table III and Fig. 6). Thus, when the group as a whole is considered, a measure of the "approximate physiological limit"[13] was not obtained in the first sitting.

Of greater interest in its bearing upon the comparison between the two senses is the fact that the practice effect is greater for touch than for hearing. In the case of touch the score of the second sitting shows an increase of 4.11% over that of the first (Table III); in the case of hearing there is an increase of 2.03%. These figures are based upon total scores. When increments 5 and 2 are eliminated from the score, on the argument that with both touch and hearing these are well below the threshold (see Fig. 6), the practice effect for touch holds nearly the same relation to that for hearing as it does when total scores are considered. The average percent of increase of Sitting II over Sitting I in the hearing condition when the first 3 increments are used to make up the score is 3.24%; in the case of touch it is 5.20% (Table III).

When individual scores are studied instead of group averages it is found that in the hearing condition 17 of the 30 *S*s improved in the second sitting; 12 showed a lowering of score and 1 remained the same (see Table V). In the touch condition 23 *S*s improved in the second sitting and 7 showed a drop in score. This suggests that the greater practice effect of the group score for touch as compared to hearing is the result, not of a few scores of large variation, but of the fact that a larger number of *S*s failed to reach the "approximate physiological limit" in the first sitting than was the case in the hearing condition.

In the first sitting 22 of the 30 *S*s made better scores by hearing than by touch (see Table V). These 22 *S*s, as a group, made in the

[13]Seashore, *The Psychology of Musical Talent*, 51.

hearing condition a score which exceeded by 184 correct judgments the score which they made in the touch condition. Four *S*s made in the touch condition a score which exceeded by 14 correct judgments the score which they made in the hearing condition. The 4 remaining *S*s scored equally under the two conditions. In the second sitting 18 of the 30 *S*s showed superiority in hearing. These 18 *S*s, as a group, made in the hearing condition a score which exceeded by 159 correct judgments the score which they made by touch. Eleven *S*s made in the touch condition a score which exceeded by 30 correct judgments the score which they made by hearing, and one *S* scored equally under the two conditions. Thus, in the second sitting the superiority of hearing over touch is less both in number of score points and in number of subjects than is the case in the first sitting.

Seashore states that ordinarily 200 judgments from an *S*, that is, the score when both sides of the phonograph record are presented twice, will give a reasonable reliability,[14] or, in other words, will give the "approximate physiological limit." His statement, of course, is based upon judgments made by *S*s tested by hearing in the usual way. In the present experiment the presentation of stimuli in the hearing condition through the vibrating diaphragm of the receiver is perhaps less favorable than the usual method of presentation directly from the phonograph. Such factors as the 'background' noise and the greater novelty of the situation may tend to raise the threshold, at least during the early part of the experiment. If such is the case it is likely that the "approximate physiological limit" will not be reached within the 200 judgments. However, when the scores of the 30 *S*s, in the hearing condition, using the average of the two sittings, are compared with the Seashore norms it is found that the distribution is but slightly lower than that of the norms. Seven *S*s rank below the 25th percentile, 10 between the 25th and 50th, 5 between the 50th and 75th, and 8 above the 75th.

In the case of touch it is even more likely that 200 judgments are insufficient. The situation is more novel, the set-up more complicated. *S* starts the experiment with a greater background of experience in judging time intervals by ear than in judging by finger-tip. The curious vibratory sensation experienced when the thumb is in contact with the diaphragm, the sound-proof box, the distracting electric buzzer, may all tend to raise the threshold to a greater degree than do the less distracting factors of the hearing condition. The greater practice effect found for touch may be explained on these grounds. It is conceivable that the superiority of hearing over touch may be explained on the same grounds.

In view of these facts one must conclude that while within the limits of this experiment the discrimination of short intervals of time by hearing is more accurate than by touch, the evidence is insufficient to state that the difference is a significant one. Further experimenta-

[14] Seashore, *Manual*, etc., 10.

tion is needed to determine whether or not with sufficient practice the ability to discriminate short intervals of time by touch may more nearly approach in accuracy the ability to discriminate the same by hearing, or even equal or exceed it.

How small an interval of time can be distinguished by touch? How does touch compare with hearing in this respect? Fig. 3 shows that when average group scores are the basis of comparison, increment 9 is below the conventional 75% threshold in the touch condition, but above the threshold in the hearing condition. In the touch condition, when the *S* must discriminate between the standard 1-sec. interval and an interval of 1.09 sec., the group as a whole judged correctly in 72.33% of the trials; in the hearing condition in 77.12% of the trials (Table II). For both touch and hearing increments 5 and 2 are well below the threshold. When the two sittings are considered separately it is found that the group score for increment 9 in the second sitting of the touch condition is only 0.4 of 1% below the group score for the same increment in the first sitting of the hearing condition, (Table III and Fig. 6). The former is barely below the threshold, the latter barely above it. It is probable that with added practice the score for touch for increment 9 would rise above the threshold.

Table IV gives further information concerning the thresholds. In the hearing condition with increment 9, in the first sitting 20 *S*s scored above the threshold; in the second sitting there were 21, a gain of only one *S*. In the touch condition with the same increment 14 *S*s scored above the threshold in the first sitting and 18 in the second, a gain of 4 *S*s. In the case of increment 5 in the hearing condition, 4 more *S*s scored above the threshold in the second sitting than in the first; in the touch condition there was a gain of 3 *S*s. In the case of increment 2 under both the hearing and touch conditions fewer *S*s scored above the threshold in the second sitting than in the first. From these figures the two conditions may be compared on the basis of individual scores rather than group averages. They show that for both touch and hearing in the case of increment 2 there is almost total lack of discrimination, and that the degree of practice given within the limits of this experiment does not improve the score; that in the case of increment 5 discrimination improves with practice and the very slight difference between the two conditions is in favor of hearing; that in the case of increment 9 touch gains more with practice than does hearing and in the second sitting very nearly equals the record of hearing in the first sitting. These facts bear out the comparison made on the basis of group scores as shown in Fig. 6.

Does the *S* who scores high in hearing also score high in touch? The scattergram, Fig. 7, shows the degree of correlation between the two conditions, using the average scores for the two sittings. Be-

cause there were only 30 *Ss* the *r* was not computed. There were no *Ss* who scored markedly high in touch and low in hearing. In 4 cases the hearing score exceeded by 10 points or more the score made by touch; the difference was greatest in the case of *S* 13, who ranked highest in the group in hearing with a score of 89.5 and only sixth in touch with a score of 68.5. It is of interest that *S* 13 is the only one of the group who had served repeatedly in work with the teletactor prior to the present experiment. His record is perhaps an argument against the suggestion, made above, that the greater novelty of the touch condition might explain the inferiority of touch when compared with hearing.

Supplementary Experiment

The problem of controlling the factor of intensity for the two conditions, touch and hearing, has been discussed above. Reasons have been given for the choice of method, that of allowing each *S* to select in the preliminary practice period that degree of amplification which seemed optimal to him for touch and hearing respectively. All *Ss* chose a higher amplification for touch than for hearing. As explained above, this does not necessarily mean a more intense stimulus, for the reduction of the amplitude of vibration, by the pressure of the thumb upon the diaphragm, lowers the intensity in the touch condition. The criticism may be raised, however, that the added amplification for touch may have more than compensated for the deadening effect of the thumb. In this case touch would have had an advantage in the comparison. This might conceivably explain why the superiority of hearing over touch in the judgment of intervals of time was as small as the results show. With this question in mind a supplementary experiment was undertaken, whose purpose was to discover the effect of changes of intensity of the stimulus upon the ability to discriminate short intervals of time through the finger tip.

Procedure. Three *Ss*, graduate students in psychology, who had not served in the previous experiment, were presented with stimuli from the phonograph record under 3 different degrees of amplification, Amplifications 1, 2, and 3. The apparatus and general procedure was the same as in the previous experiment. The amplification used was not known to *S*. It was raised or lowered by *E* according to a prearranged order after every 5 or 10 trials.

In the previous experiment there had been some indication of serial learning: a few *Ss* reported that in the second sitting they could remember the order of occurrence of the 'Longers' and 'Shorters' in some parts of the record, at the beginning of a new column especially. To prevent this possibility in the supplementary experiment the stimuli were presented in a mixed order. To present all stimuli under all three degrees of amplification it was necessary to repeat three times. This would have given an even greater opportunity for serial learning than that present in the earlier work, if the same order had been used throughout. By means of the switch between the phonograph and the amplifier any part of the record could be cut out or presented to the subject at will. The record was

run from beginning to end without pause; *E* with ear close to the needle was able to count each group of clicks as it appeared and cut out any parts desired. The omitted parts were presented to *S* at a later time.

The entire A-side of the record was used and the last 10 trials of the B-side. This included all trials with increments 20, 14 and 9. The key to the order of presentation is as follows:

I	II	III	IV	V
A a 2		A a 1	A a 3	
A b 1	A b 3			A b 2
B a 3	B a 1	B a 2		
B b 2			B b 1	B b 3
C a 3	C a 2			C a 1
C b 1		C b 3	C b 2	
D a 2	D a 3	D a 1		
D b 3			D b 2	D b 1
E a 1			E a 2	E a 3
E b 3	E b 1	E b 2		
J a 2	J a 1	J a 3		
J b 1			J b 3	J b 2

The Roman numerals at the top of the vertical columns indicate the successive presentations of the entire record. The capital letters indicate the vertical columns in the key to the records given in the preceding experiment. The small letters indicate the first or second half of a column. The Arabic numerals indicate the degree of amplification. Thus, in the first presentation of the record (Column I) *S* was given all trials in the natural order of the record without cutting out any parts. The first 5 trials of Column A were given under Amplification 2; this was followed by the second 5 trials of the same column with Amplification 1. In the second presentation of the record (Column II), the first 5 trials of Column A were omitted; the first stimuli received by *S* were those of the second half of Column A, given under Amplification 3. This was followed by the first 5 trials of Column B; the second 5 of this column were omitted, etc. By this method 60 trials, 20 for each of 3 increments, were given for each of 3 different degrees of amplification, in a mixed order which made serial learning highly improbable. The *S*s recorded their judgments in continuous order, not knowing in which column they belonged. Each *S* served for two sittings. The first sitting included 2 presentations of the record, with 60 trials on the first and 30 on the second (Columns I and II in the key). The second sitting included 3 presentations of the record with 30 trials on each (Columns III, IV, and V).

Presentation of data. The records of the three *S*s are summarized below.

	Amplification 1		Amplification 2		Amplification 3	
S	Errors	% error	Errors	% Error	Errors	% error
31.	10	16.6	10	16.6	16	26.6
32.	22	36.6	20	33.3	14	23.3
33.	12	20	9	15	4	6.6

Interpretation of data. The records of *S*s 32 and 33 suggest that increase in amplification brings an increase in accuracy of judgment. In both cases the lowest intensity gave the lowest score, and the highest intensity gave the best score. The record of *S* 31, however, shows the reverse. The poorest score was made with the highest intensity, while Amplifications 1 and 2 gave equally good results. It is of interest that the objective performance of each *S* corresponded to his subjective estimate. *S* 31 reported that in Amplification 3 the 'background' vibrations confused the issue and made judgment more difficult than in the case of the lower intensities. *S* 33 felt sure his judgments were most accurate with the highest intensity. *S* 32 reported that while the 'background' vibrations were very marked in Amplification 3 she felt that her judgments were more accurate than with the lowest intensity; she was unable to decide whether Amplification 2 or 3 appeared optimal.

One cannot base sweeping conclusions upon the records of three *S*s, but the data at hand would seem to justify the method adopted in the preceding experiment, that of allowing each *S* to choose the degree of amplification which appeared best for him individually. The influence of intensity appears to be a matter of individual differences. In this case the use of a higher amplification for touch, assuming that this involves a higher intensity of stimulation, and that the higher amplification more than compensated for the deadening effect of the thumb, would not mean for all *S*s an advantage for touch in comparison with hearing.

THE CORRELATION OF INTELLIGENCE SCORES AND CHRONOLOGICAL AGE FROM EARLY TO LATE MATURITY

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The high correlation between intelligence age and chronological age during childhood and youth is a well-demonstrated fact. Intelligence scores or mental age norms based on the results of adequately validated tests are available for each year, often for each month of each year from the period of school entrance up to 'adulthood.' Adult levels are further differentiated into 'average' and 'superior' or even into IQ or corrected IQ ratios as in the Stanford-Binet test; into 'intelligence ratings' A, B, C, D, with their plus and minus subdivisions, as in the Army Alpha tests; into 'brightness indices' or derived IQ, as in the Otis group tests; or into percentiles of adult performance, as in the Otis tests, the National Tests, and the Thorndike Intelligence Examination for High School Graduates. Studies of intelligence have for the most part been made on the more available people: school children, college students, or comparatively young adult groups, such as institutional groups or the draft army and its sub-groups. Studies of occupational groups, drawn generally from younger adult populations, sometimes include intelligence ratings, but only very rarely is it possible to discover the age ranges or age means for these groups. Comparatively little has been reported concerning the intelligence of older adults. In test standardization a plateau of ability 'adult intelligence' is generally assumed extending onward from the high point reached somewhere between the ages of 13 and 20 years.

A number of writers have discussed the probable course of the intelligence score curve in adulthood.[1] Investigations have been published with data based

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[1]E. L. Thorndike et al., *Adult Learning*, 1928, chap. xii; R. Pintner, *Intelligence Testing*, 1931, 86; F. A. Moss, *Applications of Psychology*, 158-170, 1929; H. L. Hollingworth, *Mental Growth and Decline*, 1927, 306-322; *The Psychology of Functional Neuroses*, 1920, chap. xii; C. Spearman, *The Abilities of Man*, 1927, 371-375.

on special groups which showed (1) a slight decline in score by the fiftieth year,[2] (2) a large decline at or after three score years and ten.[3] But by reason of the select character of these groups the investigators have hesitated on the basis of their findings to point a general intelligence decline of the total population from early maturity onward. Examination of 'normal adults' by the Stanford-Binet or the Point Scale technics has led examiners to question whether the often surprisingly low scores,[4] especially in the later decades, are representative of the adult maximum mental age of the individuals in question, *i.e.* whether in many cases where specific 'mental deterioration' is by no means to be diagnosed, individuals score lower on intelligence tests in later life than they once did.

The studies of the intelligence scores of adults who, by chance or on the basis of careful selection, more or less represent the normal population give certain indications as to the probable form of the intelligence curve from the age of 20 to 50 years,[5] and it is doubtless on the basis of such results as these that hypothetical curves of intelligence have been drawn.[6] Studies of atypical groups (prisoners, psychotics, general hospital and old people's home inmates) giving intelligence scores for these special populations in advanced age include the 7th, 8th, and 9th decades. The studies of the normal groups indicate either that up to the age of 50 years intelligence declines slightly from a maximum in the early 'twenties' or at least that it does not rise after such a maximum. The studies of the atypical groups indicate that among these less favored individuals a steady and considerable decline is registered from the age of 50 to 90 years, but the scores at all ages for the populations from which these individuals are drawn indicate that the individuals in question were always mentally inferior. For example, the average mental age of both Mursell's and Foster and Taylor's subjects in the 'fifties' is below 11 yrs. 6 mos.

The coefficients of correlation between score and age that we have found in the literature are for the most part limited to the normal adult ages 15 to 50.

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[2]The most extensive of these are perhaps the following: R. M. Yerkes, Psychological examining in the United States Army, *Memoirs Nat. Acad. Sci.*, 15, 1921, chap. XIV; M. V. Cobb and R. M. Yerkes, Intellectual and educational status of the medical profession as represented in the United States Army, *Bull. Nat. Res. Council*, 1, 1919-1921, 457-532; Alonzo E. Grace, The mental ability of adults as related to interests, needs, and activities, *Eightieth Annual Report Cleveland Board of Educ., Div. Adult Educ.*, 1930.

[3]J. C. Foster and G. A. Taylor, The applicability of mental tests to persons over fifty years of age, *J. Appl. Psychol.* 4, 1920, 39-58; M. F. Beeson, Intelligence at senescence, *ibid.*, 4, 1920, 219-234; G. R. Mursell, Decrease in intelligence with increase in age among inmates of penal institutions, *J. Juv. Res.*, 13, 1929, 197-203; Louis Wender, The applicability of Binet-Simon tests in psychoses of the senium, *N. Y. Med. J.*, 101, 1915, 448-454.

[4]S. L. Pressey and L. W. Cole, Are the present psychological scales reliable for the examination of adults? *J. Abnor. Psychol.*, 13, 1918-19, 302 ff. R. M. Yerkes and J. C. Foster, *A Point Scale for Measuring Mental Ability*, 1923, 81 ff.

[5]Important data have been gathered by Harold E. Jones (presented by Thorndike et al., *op. cit.*, and in an unpublished M. A. thesis by H. H. Hsiao, The performance on the Army Alpha as a function of age, *Columbia Univ.*, 1927. These give a score curve for a carefully selected representative New England population, ages 12 to 55. But the question has been raised by the investigators as to the consistently representative character of the groups in the later decades studied. (See Hsiao's discussion.) The other principal sources are enumerated in note 2.

[6]Pintner, *op. cit.*, 86; Moss, *op. cit.*, 159.

They range from −0.011 to −0.35. The coefficients (not reported) for Mursell's prisoners and for Foster and Taylor's general hospital patients, covering as they do a far wider range, would probably be larger than any of these. From the slope of the two curves we estimate them to be between −0.40 and −0.50. But, as the authors point out, neither of these populations can be thought of as representative of the general population and both are made up of individuals mentally inferior at their best. The extent to which the trend exhibited by these atypical groups, as well as by the normal populations referred to, is characteristic of other groups, especially representative normal populations, will in part appear in our further discussion.

Plan of present study. In the following pages, tables and curves are presented and described which indicate the trend from childhood to old age of intelligence scores in samplings from the populations of two American towns. In order to find the relation between age and intelligence score throughout the life span, and especially in maturity, the intelligence was measured of persons from every decade and semi-decade beginning with the 'teen' age and continuing to senescence.

An effort was made to secure a sampling of the literate citizens in each of the two towns that would (1) present sufficiently large numbers at each age decade and semi-decade for statistical reliability and that would (2) be homogeneous in general mental character from age to age. Conclusive evidence regarding the relation of age to intelligence test score will, of course, not be available until scores have been obtained year after year from childhood until old age from the same members of large representative populations. In the meantime, if as far as possible the older, the middle aged and the younger members of the same social groups can be tested in sufficient numbers and selected in sufficient numbers at each point on the age scale a substitute comparison of some value can be made. The present study is an attempt in this direction. To what extent the trend in the populations examined is characteristic of the expected trend in a representative 'general population' will be indicated as far as possible by an analysis of the characteristics of the particular groups investigated.

Subjects. The results presented are based on the scores of 823 *Ss* from two cities, A and B. In City A (a manufacturing, industrial, and banking town, population about 12,000, situated in an agricultural community, the capitol of its county) usable intelligence tests score were obtained from 122 adults (36 male, 86 female) ages 50 to 90. The distributions by age in semi-decades are given in Table I. In City B (a residential and banking town, population about 15,000, also situated in an agricultural region, the seat of several private schools and near a university) usable scores were obtained from 134 children and youths (48 male and 86 female), ages 7 to 19 inclusive, and from 567 young adults and adults (249 male and 318 female), ages 20 to 94 inclusive (cf. Table I.) The method of securing the *Ss* is described elsewhere.[7]

Test instrument. The test used was a specially printed form in large type (12-pt. Antique) of the first 60 items in the Otis Self-Administering Test of Intelligence, Higher Examination Form A given as a 15-min. test.[8] We have

[7] W. R. Miles, Correlation of reaction and coördination speed with age in adults, this JOURNAL, 43, 1931, 380, note 13.

[8] C. C. Miles, The Otis S-A Higher Examination as a fifteen-minute intelligence test, *Personnel J.*, 10, 1931, (in press).

found that scores on Form A with a 15-min. limit correlate $+0.86 \pm .02$ with scores on the same form used as a 30 min. test in the limited range of superior ability represented by 121 university students. Fifteen-minute scores of the same students correlate $+0.90 \pm .01$ with their 20-min. scores on the same test. Fifteen-minute scores of 394 eighth grade pupils correlate $+0.971 \pm .003$ with their 20-min. scores. The scores for split halves of the 15-min. test correlate $+0.96 \pm .005$ for the university students. The 15-min. scores of 55 university students correlate $+0.71 \pm .05$ with their scores on the Thorndike Intelligence Examination for High School Students.[9]

TABLE I

DISTRIBUTION BY SEMI-DECADE AGE PERIODS OF 823 PERSONS SCORED ON THE OTIS S-A 15-MIN. TEST

Age Periods	City A	City B	Total
5-9	—	20	20
10-14	—	63	63
15-19	—	51	51
20-24	—	40	40
25-29	—	38	38
30-34	—	45	45
35-39	—	45	45
40-44	—	47	47
45-49	—	43	43
50-54	17	59	76
55-59	25	58	83
60-64	24	50	74
65-69	25	52	77
70-74	20	44	64
75-79	5	27	32
80-84	5	13	18
85-89	1	5	6
90-94	—	1	1
Total	122	701	823

Each test was given individually and the time measured by stopwatch.[10] The group of 823 persons whose scores have been found usable worked for exactly 15 min. each. Of these, 4 persons finished the test, having tried all 60 items.

[9]The Thorndike test was chosen as a means of testing the validity of scores based on a 15-min. test because it is "clearly a test of superior intellect and (is) probably the most discriminative which we now possess." Cf. H. E. Garrett, The relation of tests of memory and learning to each other and to general intelligence in a highly selected adult group, *J. Educ. Psychol.*, 19, 1928, 601-613.

[10]In all cases where overtime performance occurred (22 cases, av. age 54.3 yr., av. pro rata 15-min. score 28), or where the subject for any reason whatever was unable (defective vision, 3 cases) or unwilling (cause not stated, 3 cases) to begin or to continue at work for the full 15 min. the test blank was discarded and the results are not included in this study. The results for 5 other individuals had to be omitted because the age or sex had not been noted. A total of 33 cases were thus discarded out of 856 entered on our lists.

The score used is the number of correct responses. We have prepared a table for transmuting 15-min. scores into 20-min. scores[11] (Otis has already provided a 20 to 30 min. transmutation table), and on the basis of the 30-min. scores we have made use of the corresponding mental ages, percentiles, intelligence quotients, and Army Alpha scores given by Otis. An arrangement of these equivalents with interpolated values added is given as Table II.

TABLE II

OBTAINED 15 MIN. SCORES ON OTIS' S-A HIGHER EXAMINATION WITH ESTIMATED EQUIVALENTS FOR OTIS 20- AND 30-MIN. SCORES, OTIS MENTAL AGES AND PERCENTILES, AND ALPHA SCORES

Otis S-A 15-min. score	Otis S-A 20-min. score	Otis S-A 30-min. score	Otis Mental Age	Otis IQ	Otis Percentile	Alpha score
6	6	7	8-11	65	0.2	7
8	8	10	9-7	68	0.4	10
10	10	13	10-2	71	0.7	15
12	13	17	10-10	75	2	20
14	16	20	11-5	78	4	25
16	18	23	12-0	81	5	30
18	20	26	12-6	84	8	36
20	22	29	13-0	87	12	46
22	25	32	13-7	90	20	56
24	27	35	14-2	93	28	64
26	29	38	14-8	96	36	72
28	32	41	15-2	99	45	80
30	34	44	15-8	102	56	88
32	36	47	16-2	105	68	98
34	39	50	16-6	108	76	108
36	41	53	16-11	110	81	116
38	44	56	17-3	113	87	125
40	46	59	17-8	116	91	136
42	49	62	18-0	119	95	148
44	52	64	18-3	122	97	160

Procedure. The Otis 15-min. test was administered as part of a program of psychological experiments designed to investigate the mental abilities and capacities of normal persons in youth and maturity. The fact that this particular test was intended to measure 'intelligence' was not stressed. In our special printing (4-page booklet) of the first 60 items of the Otis S-A, Form A, in large type the word 'intelligence' was omitted from the title.[12]

Every *S* was tested separately. Seated comfortably at a table, with good light, and in a quiet room *S* was given a copy of the test and several sharp pencils. He read the first page which begins as follows:

[11]C. C. Miles, *op. cit.*

[12]By kind permission of the World Book Co.

Read this page. Do what it tells you to do. Do not open this paper, or turn it over, until you are told to do so. Fill these blanks, giving the date, your name, and age. Write plainly.

Then follow spaces for name, age, the date of testing. (The ages were later verified by comparison with birthdays given in another connection.) It had already been explained in every case that all information, as far as it referred to individuals, was strictly confidential.[12] The study was announced as an investigation of the relation between age and aptitudes, and as the services of the *S*s were paid for on the basis of age, it was understood previous to registering for the appointment that this item was to be furnished. Practically no difficulty was encountered in conducting the examination.

Below the blank spaces appeared this paragraph:

Questions of different kinds are to be read and answered. Four samples are given. Here is a sample question already answered correctly. Notice how the question is answered.

Detailed instructions make up the remainder of the printed matter on the face of the booklet. These include samples of all the types of items in the test with clear and complete specifications for and practice in filling the blanks in each case. The concluding paragraph reads:

There is a total of 60 questions. You are not expected to be able to answer all of them, but do the best you can. You will work for 15 minutes after the signal to begin. Try to get as many right as possible. Be careful not to go so fast that you make mistakes. Do not spend too much time on any one question. If you can't get it go on to the next question. Don't ask questions until you have finished. *Write answers plainly, make all letters like printed capitals. Do not turn this page until you are told to begin.*

As soon as the examiner observed that the subject had finished reading this page, she said quietly and pleasantly: "Turn the page and answer the questions beginning with number one." Nothing was said about speed or accuracy, although the instructions are explicit in regard to the desirability of both these points. It seems indicative of the friendly rapport which was noteworthy throughout the entire investigation that only 3 persons out of 856 refused to perform the task in question.

The timing was done inconspicuously and without the knowledge of the *S*. At the close of just 15 min. from turning the page to item 1 the examiner returned and said: "That will be enough on this exercise, now I want you to come into the other room with me to see something new." She took note of the exact point (except in the 22 cases discarded on this account) which the *S* had reached at 15-min. as there were often individuals, especially among the older ages, who wished to continue on the item then under consideration until it was either solved to their satisfaction or given up. Some complimentary or friendly remark was usually added as *S* was guided into another room for the next part of the experimental program.

We lay stress on the matter of procedure in this study because of the difficulty which we and others have encountered with adult *S*s in administering 'intel-

[12]This confidence has been carefully guarded.

ligence' tests.[14] The willing coöperation secured in the present investigation was due to many factors, of which we wish especially to note here the coöperative interest of the club and other organization chairmen and the church officials. These people secured the aid of their members in "a coöperative study" and at all points in the investigation the *Ss* gave evidence of an attitude of willing aid in a joint community inquiry.

Results

The mean scores for decades and semi-decades in the populations of the two cities appear in Tables III–VI inclusive. Fig. 1 gives the unsmoothed intelligence score curves based on the means for the 5- and 10-yr. periods. The scores from City B extend from the earliest age testable by this type of examination throughout the life span. Those from City A extend only through later maturity, from 50 to 90 yrs. of age.

For discussion we have divided the curve into three somewhat arbitrarily selected periods: (1) The period of growth, from 7 or 8 to 18 or 19 yr.; (2) the period of early maturity, from 20 to 49 yr.; and (3) the period of later maturity, from 50 to 89 yr. It is not always possible to conduct the discussion strictly within the limits of this classification, but we have done so as far as was practicable. The results for these periods are presented in sequence.

Childhood and youth. The period of growth extends here to the age of 18 yr. The correlation between age and score from the earliest age tested until the maximum mean score is reached for this population (ages 7 to 17 yr. inclusive) is $+0.80 \pm .023$. This coefficient indicates the usual amount of agreement between score and age expected in groups of this size for the period in question and the score scatter shows no special selection of more or less superior individuals in one age as compared to another.[15] The degree of brightness in the younger group and the homogeneity of mental talent for the early period is found in a comparison of the IQ at successive age levels, and in a comparison of the average scores (using 30-min. equivalents) with Otis' norms for the ages in question. For this comparison we have combined the scores for the ages up to 19 yr. into 2-yr. periods (see Table VII) and have discarded those periods

[14]The experiences of a number of other investigators on this point have been summed up by H. S. Conrad, The measurement of adult intelligence and the requisites of a general intelligence test, *J. Soc. Psychol.*, 2, 1931, 72 f.

[15]Scores on examination A (Army) correlated +0.70 with the chronological ages of 1,162 California school pupils, aged 8 to 16 yr. Scores correlated +0.83 with school grade and +0.82 to +0.87 with Stanford-Binet mental age. (Cf. Yerkes, *op. cit.*, 333-334.)

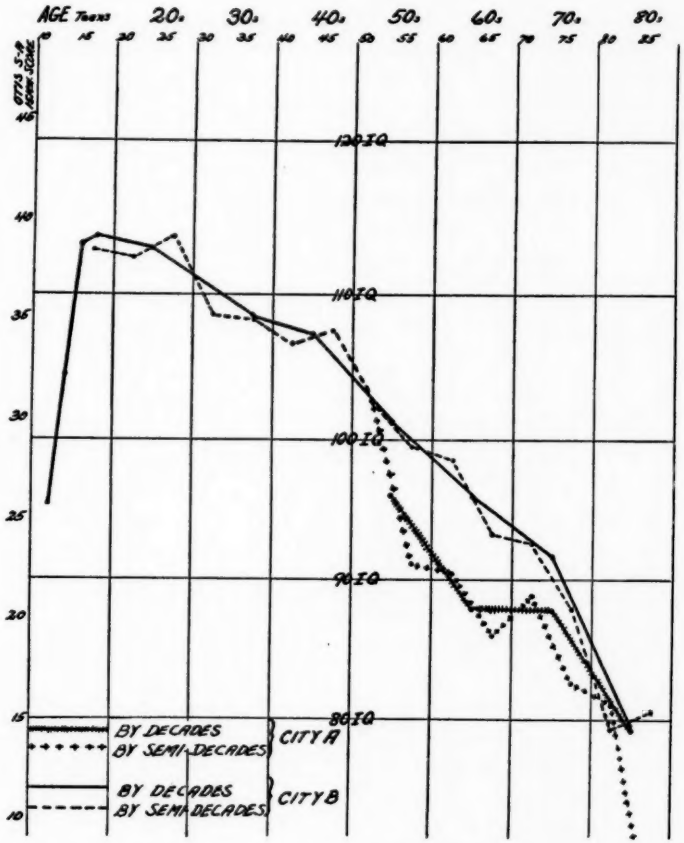

FIG. 1. THE MEAN INTELLIGENCE SCORES PLOTTED BY DECADES AND SEMI-DECADES FOR THE POPULATIONS OVER 12 YEARS OF AGE STUDIED IN TWO CITIES WITH RESULTS FOR BOTH SEXES COMBINED.

Initial rise during the period of growth shows Pearson r, +0.80. Decline from early to late maturity, r, −0.30 and −0.39.

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(below age 11) which contained less than 15 cases. This neglects 26 cases ages 7 to 10 yr. inclusive. In the combined age groups of 11 and 12 yr., 13 and 14 yr., etc. are included children ranging in age from 11 yr. 0 mo. to 12 yr. 11 mo., or 13 yr. 0 mo. to 14 yr. 11 mo. We have therefore assumed that the average ages in the respective groups are 12 years 0 months, 14 years 0 months, etc., and have compared the mean scores transmuted into 30-min. scores with the Otis norms for these ages.

TABLE III

OTIS S-A INTELLIGENCE SCORES, BY SEMI-DECADES, OF 124 ADULTS, BOTH SEXES, FROM CITY A

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Semi-decades	Means	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
18	50-54	30.94	8.92	2.76	45.5	15'10	104	60	92
25	55-59	22.86	10.16	0.28	33.3	13'10	91	23	60
24	60-64	22.18	6.82	1.32	32.0	13'7	90	20	56
25	65-69	19.10	9.40	−0.74	27.5	12'10	86	12	43
20	70-74	21.30	10.24	1.02	31.0	13'5	89	18	56
5	75-79	16.90	8.08	0.20	24.4	12'2	82	6	33
6	80-84	15.82	10.28		23.0	12'0	81	5	30
1	85-89	5.50			6.0	8'1	64	0.2	6

TABLE IV

OTIS S-A INTELLIGENCE SCORES BY DECADES AND BY 20-YR. PERIODS OF 124 ADULTS, BOTH SEXES, FROM CITY A

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
43	50-59	26.2	10.6	2.62	38.1	14'8	96	37	73
49	60-69	20.6	9.8	.84	29.9	13'3	88	16	54
25	70-79	20.4	9.7	1.55	29.7	13'2	87	15	53
7	80-89	14.5	10.0		21.0	11'6	79	4	27
92	50-69	23.3	10.5	1.98	33.2	14'0	91	25	60
32	70-89	19.1	10.2		28.0	12'10	86	11	44

The mental age equivalents for the three age levels, 12, 14, and 16 yr., are respectively 14'8, 16'2, and 17'4 yr. They show gains of something over a year for each 2-yr. period. The peak of ability is reached in the fourth 2-yr. group (see Table VII). This group

TABLE V

OTIS S-A INTELLIGENCE SCORES, BY SEMI-DECADES, OF 617 ADULTS, BOTH SEXES, FROM CITY B

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Semi-decades	Means	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
51	15-19	38.50	8.04		57.0	17'5	114	88	126
				0.25					
40	20-24	38.10	7.04		56.0	17'3	113	87	125
				−0.66					
40	25-29	39.22	8.20		56.9	17'4	115	89	131
				1.94					
43	30-34	35.26	10.32		50.8	16'8	109	77	110
				0.09					
44	35-39	35.06	8.44		50.0	16'7	108	76	108
				0.59					
48	40-44	33.82	11.36		48.8	16'5	107	73	104
				−0.29					
42	45-49	34.50	11.04		50.5	16'6	109	77	109
				1.80					
63	50-54	30.98	11.64		45.5	15'11	104	62	92
				1.41					
56	55-59	28.74	9.04		42.0	15'4	100	50	83
				0.41					
50	60-64	27.94	11.16		41.0	15'1	99	45	80
				1.87					
53	65-69	24.22	9.64		35.3	14'3	95	29	65
				0.21					
42	70-74	23.78	10.48		34.7	14'1	93	27	63
				1.42					
26	75-79	20.46	8.56		29.8	13'0	88	15	54
				1.98					
13	80-84	14.50	9.40		20.8	11'6	79	4	26
				−.016					
5	85-89	15.30	9.60		22.0	11'10	80	5	28
1	90-94	15.50	0		22.2	11'11	80	5	28

TABLE VI

OTIS S-A INTELLIGENCE SCORES, BY DECADES, AND BY 20-YR. PERIODS OF 616 ADULTS, BOTH SEXES, FROM CITY B

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
51	15-19	38.5	8.0		55.9	17'3	114	88	126
				−.11					
80	20-29	38.7	7.9		56.0	17'3	114	88	126
				2.53					
87	30-39	35.1	9.6		50.6	16'8	109	77	108
				.17					
90	40-49	34.2	9.2		50.0	16'6	108	76	108
				3.27					
119	50-59	29.9	9.7		44.0	15'8	102	56	88
				2.86					
103	60-69	26.1	10.6		38.0	14'8	96	37	72
				2.05					
68	70-79	23.1	8.7		33.5	13'11	92	24	61
				3.54					
18	80-89	14.7	9.3		21.0	11'7	79	4	26
131	15-29	38.6	7.9		55.9	17'3	114	88	126
				3.84					
177	30-49	34.7	10.4		50.0	16'6	108	76	108
				6.51					
222	50-69	28.1	10.4		41.0	15'2	99	46	80
				5.48					
86	70-89	20.9	10.2		30.5	13'4	88	17	55

(18 yr.) shows a gain of only one month (17·5) over the 16-yr. level. The high mental ages of all four groups (as compared to their chronological ages) show that the populations tested are above average. How much above is indicated, or at least suggested, by the equivalent Otis IQ of 115, 115, 117, 115.[16]

During the period of growth we are then dealing with a superior and homogeneous population whose Otis IQ approximates 115 and whose mental development follows the usual upward trend in score expressed in the coefficient +0.80 ±.023.

TABLE VII

OTIS S-A INTELLIGENCE SCORES OF 97 'TEEN' AGE REPRESENTATIVES, BOTH SEXES, OF CITY B

			Otis 15-min. S-A Score			Equivalent Measures				
No. Cases	Age Group	Av. Age	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
22	11-12	12	25.8	8.7	−2.96	37.8	14·8	115	89	74
35	13-14	14	32.3	7.0	−2.69	47.4	16·2	115	89	99
23	15-16	16	38.9	10.4	−0.11	56.4	17·4	117	89	129
17	17-18	18	39.2	7.7		56.6	17·5	115	89	130

Early maturity. This period we count as extending from the third to the fifth decade. The curve of intelligence score averages maintains almost the same level throughout the twenties. It drops very slightly in the 'thirties', and but little more in the 'forties.' The average for the 'twenties' has an equivalent mental age of 17 yr. 3 mo. (Otis IQ 114). The population is, therefore, approximately equal in mental level to the 17-18 yr. old group and in brightness to the other groups representing the period of growth. Homogeneity of mental talent seems therefore to be demonstrated up to this point as far as this population is concerned.

The averages for the decades from the third to the fifth are presented in Table VI; those for the semi-decades in Table V. These tables present beside the means and the σ for the groups in question, (1) the significance of the differences between successive periods

[16]P. Cattell has shown that "at all the Stanford-Binet IQ levels the Otis IQ (Form A) tends to be about six points lower than that of the Binet; P. Cattell, IQs and the Otis measure of brightness, *J. Educ. Res.*, 22, 1930, 31-35. If a correction is made in these terms on our figures the resulting IQ level would be 120 to 123 indicating even more pronounced superiority than the original Otis equivalents.

and (2) the score equivalents period by period in terms of Otis 30-min. score, mental age, IQ, percentile, and Army Alpha score.

From the values in Table VI it may be seen that the mean score and mental age for the 'twenties' is not lower than that for the previous semi-decade. The equivalent mental ages are the same, 17 yr. 3 mo, and the Otis IQ of 114 registered by both is approximately the same as the average for the members of the population under 20 yr. of age. The drop from the third to the fourth decade mean is represented by a decline of 7 mental age months, from 17 yr. 3 mo. to 16 yr. 8 mo. The difference between the two means is two and a half times its σ and therefore of possibly true significance. Between the 'thirties' and the 'forties' the drop is only one mental age month and the decline can not be regarded as in the least significant. From the 'forties' to the 'fifties', however, a drop of 11 mental age months is registered and the difference between the two means is more than three times its σ and, therefore, very probably significant statistically.

The semi-decade indices (Table V) show with somewhat greater detail the trend traced in the previous paragraph. It appears from these figures that the score curve forms a plateau from the late 'teens' through both early and late 'twenties,' that a considerable, but not statistically significant, drop occurs between the late 'twenties' and the early 'thirties' (8 mental age months) and also between the late 'forties' and the early 'fifties' (7 mental age months). Otherwise a gentle decline is usually present from one 5-yr. period to the next. Registered in mental age months the loss indicated from the high point of 17 yr. 4 mo. in the late 'twenties' to 15 yr. 11 mo. in the early 'fifties' is 17, making an average of 3.4 mental age months in each 5-yr. period. From Table VIII, it may be seen that although the successive semi-decade losses in intelligence scoring ability in early maturity are not significant statistically for groups of the present size those between alternate decade levels are definitely so, and the difference between the third and the sixth decade means is almost 7 times its σ.

The Otis IQ in the decade periods between 20 and 50 yr. of age are as follows: 114, 109, 107. If the population groups represented at the successive 10-yr. periods are homogeneous it appears from these IQ that within the age period in question a drop of about 7 IQ points has taken place. The Otis IQ for the 'fifties' shows a further drop to 102, five IQ points below the 40-yr. level.

The coefficient of correlation between life age and score during this period gives a further measure of the decline in scoring ability. The coefficients for various parts of the age span and for the total range from the late teens to the 'nineties' appear in Table IX. To show the relation of score to age in early maturity and for comparison with other indices of this sort published by other writers (or coefficients which we have been able to compute from their published

TABLE VIII

CORRECTNESS OF THE DIFFERENCES BETWEEN MEAN DECADE INTELLIGENCE SCORES

(Both Sexes)

	Decade means compared	D/σ diff.	Decade means compared	D/σ diff.
City A	50-59 70-79	2.29	50-59 80-89	5.89
City B	15-19 30-39	2.22	30-39 50-59	3.79
	15-19 40-49	2.92	30-39 60-69	6.19
	15-19 50-59	5.94	40-49 60-69	5.76
	15-19 60-69	8.13	40-49 70-79	7.80
	20-29 40-49	3.42	40-49 80-89	8.34
	20-29 50-59	6.92	50-59 70-79	4.96
	20-29 60-69	9.26	50-59 80-89	6.61

data) we have computed three coefficients, one for the period from 15 to 55 yr. of age (males and females combined) $r = -0.283 \pm .031$; and two for the period from 30 to 55 yr. of age as follows: males only, $r = -0.108 \pm .065$, both sexes, $r = -0.191 \pm .040$. These are given with the results of other investigators in Table X. The coefficients between score and age for the period of early maturity (age 20 to 55 or 60 yr.) range from −0.01 (Yerkes) to −0.283 (our data). A single correlation (Bingham) on the range from 20 to 63 yr. is −0.35. The question of selection suggested by these coefficients and analyzed by several of the investigators will be discussed later in the present report.

TABLE IX

CORRELATIONS BETWEEN AGE AND INTELLIGENCE SCORE IN THE AGE RANGE 15 TO 95 YEARS

	City A			City B		
Age range	Sex	No. cases	Product-moment r	Sex	No. cases	Product-moment r
16-55				M & F	368	−.283 ± .031
30-55				M	106	−.108 ± .065
30-55				M & F	249	−.191 ± .040
15-95				M & F	620	−.528 ± .021
20-95				M	250	−.465 ± .032
20-95				F	319	−.532 ± .026
20-95				M & F	569	−.504 ± .021
30-95				M	207	−.371 ± .040
30-95				F	282	−.520 ± .028
30-95				M & F	489	−.455 ± .023
40-95				M	166	−.381 ± .044
40-95				F	234	−.489 ± .033
40-95				M & F	400	−.447 ± .026
50-95	M	36	−.302 ± .103	M	128	−.392 ± .051
50-95	F	86	−.367 ± .064	F	183	−.392 ± .042
50-95	M & F	122	−.343 ± .055	M & F	311	−.385 ± .032

City A and City B combined

Age range	Sex	No. cases	Product-moment r
50-95	M	166	−.387 ± .045
50-95	F	267	−.370 ± .036
50-95	M & F	433	−.372 ± .028

From the peak, mental age 17 yr. 5 mo., of the intelligence score curve reached at approximately the age of 18 yr. the trend is first almost level, then it gradually declines to a point at the age of 50 yr. some 15 or 16 mental age months lower (mental age 16 years 1 month). The decline in Otis IQ points is from 115 to 104.5 and the correlation coefficient indicating the relation of score to age is −0.283 for males and females combined.

Later maturity. This we have considered as covering from the sixth to the ninth decade. From the age of 50 yr. onward scores for two populations, Cities A and B, are available. It is obvious from the intelligence score curves for these two cities (Fig. 1) that A and B represent different levels of intelligence. It is therefore necessary to consider them separately and not as a single larger

TABLE X

CORRELATIONS BETWEEN AGE AND INTELLIGENCE SCORE IN THE AGE RANGE 15–65 YR.

Investigator[17]	Date	Age range	No. cases	Product-moment r	Test	Subjects
	1921	21-60	146	−.011±.056		Medical officers, Camp Devens
	1921	21-60	308	−.120±.038		Infantry officers Camp Devens
Yerkes	1921	21-55	5404	−.192±.009		Medical officers, Camp Greenleaf
	1921	21-30	2475	−.063±.013		Medical officers, Camp Greenleaf
	1921	30-60	3267	−.192±.011		Medical officers, Camp Greenleaf
Filer & O'Rourke	1922-3	19-53	92	+.067±.071	Civil Service clerical test	Civil Service clerks
Bingham & Davis	1924	20-63	73	−.350±.07	15-min. spiral omnibus based on Army Alpha	Business men: executives and salesmen
Garrett	1928	Av. 19.4 yr. S.D. 2 yr.	158	−.110±.053	Thorndike exam. for H. S. grads.	College students, Sophs, Jrs., Srs., in Elem. Psych. course Columbia
Jones	1928	15-54	1130	−.110±.023	Army Alpha	Typical rural New England community
Burgard	1929	18-50+(1 at 65, 1 at 75)	60	−.151±.085*	Individual test Binet and Performance Type	Specially selected cases. Wide educational range
Conrad	1929	17-54	48	−.063±.097	Army Alpha	Typical rural New England community
& Jones	1929	17-54	58	−.239±.084	Army Alpha	
Grace	1930	15-59	599	−.100±.028*	Otis S-A	Adult school pupils, Cleveland
	1930	15-59	2400	−.190±.013	Thorndike test of word knowledge	
Miles &	1931	30-55	106	−.108±.065	Otis S-A	Males, City B
Miles	1931	30-55	249	−.191±.04	15-min.	Both sexes, City B
	1931	16-55	368	−.283±.03		

*Our computation from the tables of data.

[17]R. A. Yerkes, Psychological examining in the United States army, *Mem. Nat. Acad. of Sci.*, 15, 1921, 817-17; H. A. Filer and L. J. O'Rourke, Progression in civil service tests, *J. Person. Res.*, 1, 1922-23, 484-520; W. V. Bingham and W. T. Davis, Intelligence Test Scores and business success, *J. Appl. Psych.*, 8, 1924, 1-22; H. E. Garrett, The relation of tests of memory and learning to each other and to general intelligence in a highly selected adult

population except where trend during the age period of 50 to 90 yr. is being considered. However, it is equally clear that the score curves for both follow approximately the same downward course. The average population of one city registers at successive decades mean scores regularly divergent from those of the other and this divergence is always in the same direction, *i.e.* City A is always lower than City B. The significance of the differences is as follows for the 'fifties,' 'sixties,' 'seventies,' and 'eighties': 2.01, 3.18, 1.21, 0.55. For the combined period 50–69 yr. the ratio of difference to σ diff. is 3.75 and for the total period, 50–90 yr., it is 3.48.

The decade and semi-decade scores for City B, forming the curve which we have followed up to this point, (Tables V and VI, and Fig. 1) show about the same rate of decline from the 'fifties' onward as was indicated from the 'forties' to the 'fifties.' From the 'forties' to the 'seventies' the decade scores describe a straight line, from the 'seventies' to the 'eighties' the curve is steeper. The loss in scoring power from the 5th to the 6th decade was 11 mental age months. From the 6th to the 7th decade it was 12 mo., from the 7th to the 8th there is a drop of 9 mo., and from the 8th to the 9th, 28 mo. The differences are respectively 3.2, 2.9, 2.0, and 3.5 times their σ and, therefore, possibly or probably of statistical significance. The semi-decade drops are always less than twice the σ diff.; they range in amount, for the successive groups having more than 10 members, from 2 to 18 mental age months and average 8.5 mental age months. The total mental age loss from the age of 50 yr. (M.A. 16–1) to the age of 85 yr. (M.A. 11–7) is 54 M.A. months or 4.5 M.A. years. The Otis IQ for the 6th to 9th decades are as follows: 102, 96, 92, 79. From the age of 50 yr. (IQ 104.5) to the age of 85 yr. (IQ 79) the loss is 25.5 IQ points. The relation between scoring ability and age in later maturity (City B) is indicated further by the correlation coefficients (Table IX); for the period from 50 to 95 yr. of age, $r = -0.385 \pm .032$ (both sexes combined) and -0.392 for either sex separately.

In City A the trend is similar but the curve is, as already noted, at a lower level. The decade populations are smaller than those for

group, *J. Educ. Psychol.*, 19, 1928, 601-613; H. E. Jones in E. L. Thorndike's *Adult Learning*, 1928, 158; J. Burgard, Testreihe zur Untersuchung der Allgemeinintelligenz Erwachsener, *Arch. f. d. ges. Psychol.*, 68, 1929, 325-394; H. S. Conrad and H. E. Jones, Psychological studies of motion pictures: III. Fidelity of report as a measure of intelligence, *Univ. Calif. Publ. Psychol.*, 3, 1929, 245-276; A. E. Grace, The mental abilities of adults as related to adult interests, needs, activities, *Adult Educ. Bull., Cleveland Board of Education*, 941930, (series 4, no. 2) 99-101.

City B and the differences between levels are therefore correspondingly less significant statistically. The decade drops are respectively 2.6, 0.8, and 1.6 times the σ diff. Those for the semi-decades are with one exception (early to late 'fifties' 2.8) never even possibly of statistical significance. But gradual though they are, they total for the period from the beginning of the 6th to the middle of the 9th decade a loss of 52 mental age months or 4 1/3 mental age years, which is exactly the amount of loss suffered in City B in the same age period. The difference between the mean score for the 'fifties' and the mean score for the 'eighties' is 5.9 σ diff. The degree of correlation between score and age in City A (Table IX) from 50 to 95 yr. of age is $-0.343 \pm .055$, for the sexes combined; $-0.302 \pm .103$, for the males alone; or $-0.367 \pm .064$, for the females alone.

From the means for the populations in the two cities the decline in scoring power is seen to persist and even to increase from the beginning to the end of the period of late maturity. Differences, insignificant as the mean scores for successive 5-yr. periods are compared, become highly significant as they accumulate and the loss for the total period is found to be 7.48 σ diff. for the two cities combined. The correlation between intelligence score and age for the combined group, age 50 to 95 yr., is $-0.372 \pm .028$.

Summary. From a maximum score at the age of 18 yr. the curve of intelligence maintains almost a plateau through the 'twenties,' begins then to fall, and persists in its downward course, at first slowly, then more rapidly until the end of the life span. The drop in intelligence from the age of 18 to the age of 85 yr. (City B) measured in 15-min. scores on the Otis S-A test is 24 points, 62% of the original high score; in mental age months it is 70 (5 yr. 10 mo.), in Otis IQ points it is 36. The correlation between age and score from the age of 20 to the age of 95 yr. is approximately $-0.50 \pm .02$, and the difference between the average score in the 'twenties' (City B) and that in the 'eighties' (City B) is 9.9 σ diff.

Corollary Results

(*1*) *Brightness groups.* Besides comparison by central tendency such as that described above, a comparison of the frequency of various degrees of ability in the several age decade classes is another method of indicating the trend of intelligence scores throughout the life span in populations A and B. For this comparison we have employed the well-known Army classifications, transmuting our Otis 15-min. scores into equivalent Alpha scores for the purpose (see Table II for transmutation values). The percentage of cases in each brightness group is given

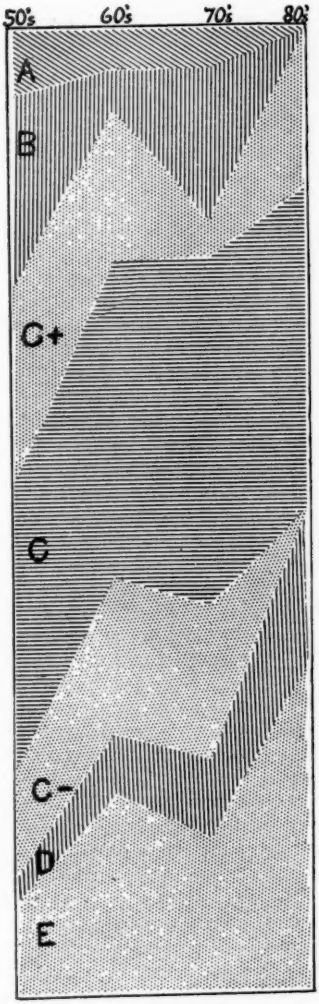

FIG. 2. PERCENTAGE DISTRIBUTIONS FOR 7 BRIGHTNESS CLASSES (ARMY) BY DECADES FOR CITY A
(122 cases)

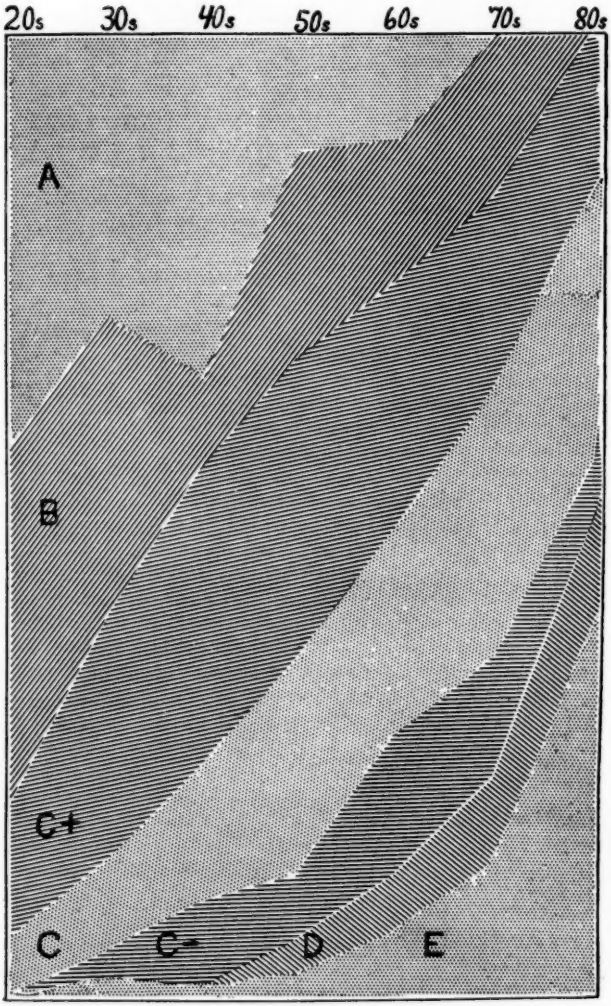

FIG. 3. PERCENTAGE DISTRIBUTIONS OF THE 7 BRIGHTNESS CLASSES (ARMY) BY DECADES FOR CITY B
(567 cases)

by decades in Tables XI and XII for City A and for City B. Table XIII gives the percentages for the sexes separately in duo-decade groupings. Figs. 2 and 3 give the decade results in graphic form. In the case of City A the comparison covers four decades from the ages of 50–90 yr. The graph for City B gives the classification from the ages of 15–90 yr., thus covering 7.5 decades.

TABLE XI

PERCENTAGE OF ADULTS, BOTH SEXES, AT EACH AGE DECADE IN 7 BRIGHTNESS CLASSES

(City A, 122 Cases)

Class	50s	60s	70s	80s
A	7.1	4.1	4.0	—
B	19.1	4.1	16.0	—
C+	21.4	16.3	4.0	16.7
C	28.6	32.6	36.0	33.3
C−	11.9	16.3	16.0	—
D	2.4	6.1	8.0	16.7
E	9.5	20.4	16.0	33.3
Total	100.0	99.9	100.0	100.0
No. Cases	42	49	25	6

TABLE XII

PERCENTAGE OF ADULTS, BOTH SEXES, AT EACH AGE DECADE IN 7 BRIGHTNESS CLASSES

(City B, 567 Cases)

Class	20s	30s	40s	50s	60s	70s	80s	90s
A	42.3	28.9	35.6	12.0	10.8	—	—	—
B	37.2	31.1	8.9	21.4	14.7	16.9	—	—
C+	14.1	25.6	31.1	30.8	25.5	19.7	16.7	—
C	6.4	10.0	14.4	23.1	21.6	28.2	27.8	—
C−	—	2.2	8.9	6.9	14.7	12.7	5.5	100.0
D	—	—	—	3.4	5.9	8.5	11.1	—
E	—	2.2	1.1	2.6	6.9	14.1	38.9	—
Total	100.0	100.0	100.0	100.2	100.1	100.1	100.0	100.0
No. Cases	78	90	90	117	102	71	18	1

The persistently lowered percentage of representatives in the higher classifications A and B with increasing age and the disappearance of these classes in both populations in the ninth decade is noteworthy. Similarly the appearance of the groups C−, D, and E, in a population (City B) where they were non-existent in the earliest adulthood, to include altogether 35% of the population in the eighth and 55% in the ninth decade, is a striking indication of the slowing down of the abilities of older people as far as intelligence test performance of the type used here is concerned.

At this point the question may well be raised: are the successive decade groups for the period of maturity homogeneous? In other words is the decrement registered (1) by the regular drop from decade mean to decade mean, or (2) by the

diminishing percentages in the upper brightness classifications and the increasing percentages in the lower levels, actually a function of increasing age or are the lower scores of the older groups due to differences of some other sort. If, for example, one sex regularly scores lower than the other and if the older age groups are disproportionate as compared to the younger with reference to their male and female constituents (not true of our populations) the decrement would be spuriously attributed to age. Or if the younger groups contain actually a larger proportion of the more able persons of the population as compared to the older another confusing element would be introduced. This latter is quite unlikely, for actually the opposite is very probably true. Length of life seems positively related to general mental ability and resistance to interference from the "weight of years" may be proportional to mental ability. The data gathered by one of the writers in a study of the intelligence of eminent men show a positive relation between longevity and IQ estimated from extensive biographical material.[18] The average IQ estimate for eminent men who lived to be 80 years of age or more was not less than 5 IQ points higher than the average IQ estimate of men of shorter lives. The correlation between IQ and longevity for the group as a whole is not very large but it is of practical significance when it is recalled that the intelligence range under consideration here was fairly narrow. Therefore, the decade decrements which we have found for the sample populations tested with the Otis S-A examination are theoretically less than would be shown if the same identical population could have been tested at succeeding 10-yr. intervals.

There may be other more obscure elements tending to produce heterogeneity in the populations from decade to decade but the present data offers no means for isolating or examining them. The question of sex differences and of the constitution of the groups in terms of educational status is, however, directly amenable to investigation.

(*2*) *Sex differences.* The score curve for the ages from 50–90 yr. in City A is based on the tests of 37 males and 87 females, the curve for the ages from 15–90 yr. in City B is derived from tests of 272 males and 344 females. The decade means for the sexes separately with equivalent scores, Otis mental age, IQ, percentile, and Army Alpha score appear in Tables XIV to XVII inclusive. In Table XVIII the statistical significance of the differences between the means for the sexes appear decade by decade for each of the cities. It is quite clear from these tabulated results that the scores for the males tend to average higher than those for the females, but they are not always higher. In the 12 comparisons 9 are higher and of these only 3 show D/σ diff. greater than 2; the highest is 2.97 which is just 'probably significant.' When the decades are combined into larger age periods, the 5 differences turn out 4 in favor of the men and 1 (age range 30–49, City B, −0.70) in favor of the women. There are 2 (3.58 and 2.61) that seem to indicate possibly or probably true superiority in the scoring ability of the men in question over the women from the corresponding age groups. As the trends are not regular (see Fig. 4) we are inclined to regard the difference exhibited between the sexes as possibly due to errors in sampling. But it will be noticed that in both cities the curve for the females drops more rapidly than that for the males from the 'fifties' to the 'sixties' and there is something of a proportional recovery after the seventh decade.

[18] C. M. Cox, The early mental traits of three hundred geniuses, *Genetic Studies of Genius*, II, 1925, Table 12 A, facing p. 60.

TABLE XIII

PERCENTAGE OF ADULTS BY 20-YR. PERIODS IN 7 BRIGHTNESS CLASSES
(618 Cases, Sexes Separate and Combined, City B)

	15-29			30-49			50-69			70-89			90-109		
Class	M	F	Both	M	F	Both	M	F	Both	M	F	Both	M	F	Both
A	55.6	35.4	45.0	30.0	34.0	32.2	15.7	8.5	11.4	—	—	—			
B	30.1	30.8	30.2	22.5	18.0	20.0	19.1	17.7	18.3	25.0	5.7	13.5			
C+	11.1	27.7	19.4	26.2	30.0	28.3	30.3	26.9	28.3	16.7	20.8	19.0			
C	3.2	4.6	4.7	11.3	13.0	12.2	19.1	24.6	22.4	22.2	32.1	28.1			
C−	—	1.5	0.7	6.3	5.0	5.6	11.2	10.0	10.5	8.3	13.2	11.2		100.0	100.0
D	—	—	—	—	—	—	1.1	6.9	4.6	8.3	9.4	8.9			
E	—	—	—	3.8	—	1.7	3.4	5.4	4.6	19.4	18.9	19.0			
Total	100.0	100.0	100.0	100.1	100.0	100.0	99.9	100.0	100.1	99.9	100.1	99.7	0	100.0	100.0
No. Cases	63	65	129	80	100	180	89	130	219	36	53	89	0	1	1

TABLE XIV

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS
(37 Adults, Males, from City A)

	Otis 15-min. S-A Scores				Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
12	50s	27.2	3.2	1.48	39.0	14'10	97	39	76
13	60s	23.1	9.4	.37	34.0	14'0	92	25	62
8	70s	21.5	9.9	.87	31.0	13'5	89	17	56
4	80s	16.5	9.2		24.0	12'2	82	6	32
25	50-69	25.1	10.0	1.51	36.0	14'4	94	30	67
12	70-89	19.8	10.0		28.2	12'10	86	12	46

TABLE XV

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS
(87 Adults, Females, from City A)

	Otis 15-min. S-A Scores				Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
31	50s	25.9	10.6	2.45	37.0	14'6	95	33	70
36	60s	19.8	9.6	−.03	28.3	12'10	86	12	46
17	70s	19.9	9.7	1.24	28.4	12'10	86	13	47
3	80s	11.8	10.5		17.0	10'10	75	2	19
67	50-69	22.6	10.6	1.48	33.0	13'10	91	22	60
20	70-89	18.7	10.2		26.5	12'6	85	9	40

TABLE XVI

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS
(272 Adults, Males, from City B)

	Otis 15-min. S-A Scores				Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
21	15-19	41.7	8.7	.30	59.7	17'9	118	93	143
43	20-29	41.1	8.2	3.51	59.	17'8	117	93	140
41	30-39	34.2	9.7	.14	49.2	16'5	107	73	105
38	40-49	33.8	12.6	.96	48.8	16'4	107	72	103
50	50-59	31.3	10.6	1.08	46	16'0	104	64	93
43	60-69	28.9	10.9	1.59	42.4	15'5	100	51	84
29	70-79	24.7	11.3	1.45	36	14'4	94	30	67
7	80-89	9.9	7.2		14	10'4	72	1	16
64	15-29	40.6	10.0	3.92	58.6	17'8	117	93	136
79	30-49	34.0	11.4	2.34	49	16'5	107	72	104
93	50-69	30.3	10.6	3.43	44.5	15'9	103	60	89
36	70-89	21.9	13.2		32.0	13'7	90	20	56

The difference between the mean scores of all of the males and all of the females of City A is 0.84 in favor of the men; it is 0.41 σ diff. The difference between the means for the sexes (ages 15 to 90 yr.) in City B is 2.76; this is 2.91 σ diff. but here it should be noted that the older (lower score) groups contain more women than men, while the younger (higher score) groups are fairly equal.

TABLE XVII

OTIS S-A INTELLIGENCE SCORES, BY DECADES AND 20-YR. PERIODS

(344 Adults, Females, from City B)

		Otis 15-min. S-A Scores			Equivalent Measures				
No. Cases	Period	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Percentile	Army Alpha
30	15-19	36.2	6.7	0	52.1	16'9	110	80	114
37	20-29	36.2	6.3	.16	52.1	16'9	110	80	114
46	30-39	35.9	9.2	.79	51.8	16'9	110	80	112
52	40-49	34.4	10.4	3.06	49.8	16'6	108	75	106
69	50-59	28.9	8.9	2.85	42.5	15'5	101	52	84
60	60-69	23.9	9.8	1.60	34.8	14'2	93	28	63
39	70-79	20.9	8.1	1.04	30.5	13'4	89	17	55
11	80-89	17.8	9.1		25.7	12'5	84	8	36
67	15-29	36.2	6.6	.78	52.3	16'9	110	81	115
98	30-49	35.2	10.0	4.26	50.8	16'8	109	77	109
129	50-69	26.6	9.9	6.31	39.0	14'10	97	38	76
50	70-89	20.3	8.5		29.5	13'1	88	15	52

For the present study a more important fact than the rather slight, but possibly characteristic sex difference, is that when the sexes are regarded separately, from decade to decade throughout maturity, each shows a considerable score decrement. This quite definite tendency has already been indicated by the coefficients of correlation for the general groups as presented in Table IX. Without very large populations the differences could scarcely be made out from year to year. When we compare one 10-yr. period with the next they are not usually large enough to be beyond statistical doubt especially in our smaller groups of data from City A. But for 20-yr. periods the changes are sufficiently large to be quite convincing.

The graphic presentation of the results for the sexes separately in the two cities, Fig. 4, shows again that the downward trend of the intelligence scores as age increases is definitely a characteristic of both sexes. A weighted score that allowed for the differences in the relative sizes of the populations for the sexes from one decade to another would therefore show the same general trend as that indicated for our populations as they stand. No matter how or from which side we picture them the facts indicate progressive decrement in the intelligence scoring ability of the human being with increasing age.

TABLE XVIII

SEX DIFFERENCES IN MEAN INTELLIGENCE SCORES OF ADULTS CLASSIFIED BY DECADE AND DUO-DECADE AGE PERIODS

(Otis 15-min. Score)

	City A					City B				
Age Periods	Sex	No. Cases	Mean	σ_m	D/σ diff.	Sex	No. Cases	Mean	σ_m	D/σ diff.
15-19	M					M	21	41.7	1.9	2.45
	F					F	30	36.2	1.2	
20-29	M					M	43	41.1	1.2	2.97
	F					F	37	36.2	1.0	
30-39	M					M	41	34.2	1.5	−0.87
	F					F	46	35.9	1.4	
40-49	M					M	38	33.8	2.0	−0.24
	F					F	52	34.4	1.4	
50-59	M	12	27.2	.93	0.61	M	50	31.3	1.5	1.33
	F	31	25.9	1.90		F	69	28.9	1.1	
60-69	M	13	23.1	2.61	1.08	M	43	28.9	1.7	2.33
	F	36	19.8	1.61		F	60	23.9	1.4	
70-79	M	8	21.5	3.50	0.38	M	29	24.7	2.1	1.53
	F	17	19.9	2.36		F	39	20.9	1.3	
80-89	M	4	16.5	4.60	0.62	M	7	9.9	2.7	−2.03
	F	3	11.8	6.08		F	11	17.8	2.8	
15-29	M					M	64	41.1	1.06	3.58
	F					F	67	36.3	.81	
30-49	M					M	79	34.0	1.26	−0.70
	F					F	98	35.1	1.0	
50-69	M	25	25.1	1.99		M	93	30.3	1.10	2.61
	F	67	22.6	1.29	1.06	F	129	26.6	.87	
70-89	M	12	19.8	2.89		M	36	21.9	2.19	0.64
	F	20	18.7	2.29	0.30	F	50	20.3	1.20	

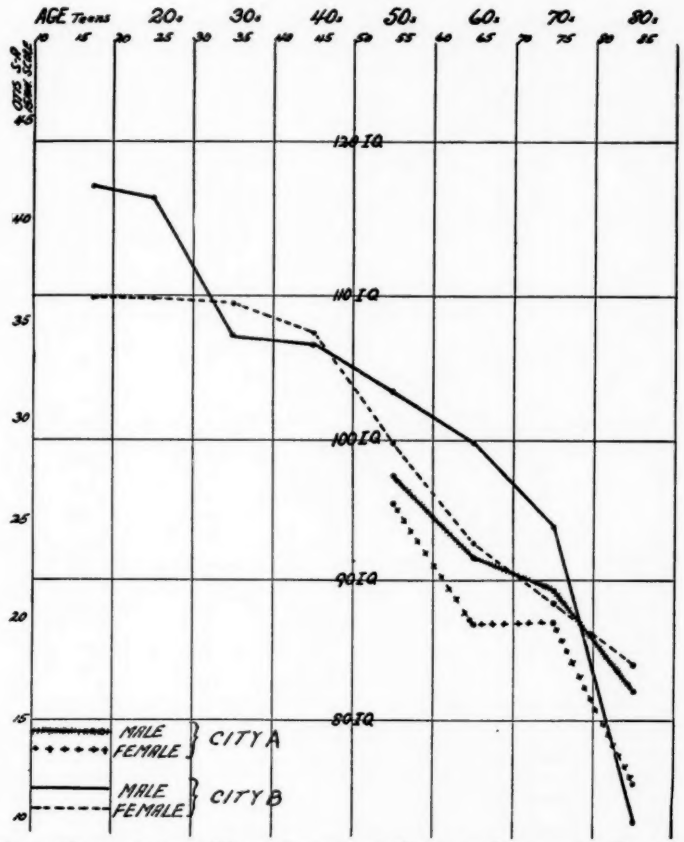

FIG. 4. DISTRIBUTION OF MEAN INTELLIGENCE SCORES BY SEXES AND BY DECADES

(3) *Educational levels.* The lower position of the mean curve for City A as compared to that for City B has been referred to. The differences between the means for the two cities at successive decades from the 6th to the 9th are 2.0, 3.1, 1.2, and 0.06 times the respective σ diff. When the two populations were analyzed as to educational status an explanation was found in the lower educational rating of the individuals in City A compared to those in City B. In our tested population from City A there were 92 persons who had given answers about their education which made possible classification into one or another of three groups: grade school (0 to 8 years); high school, normal or business course (9 to 12 years of schooling); college (1 to 10 years of college training). In City B

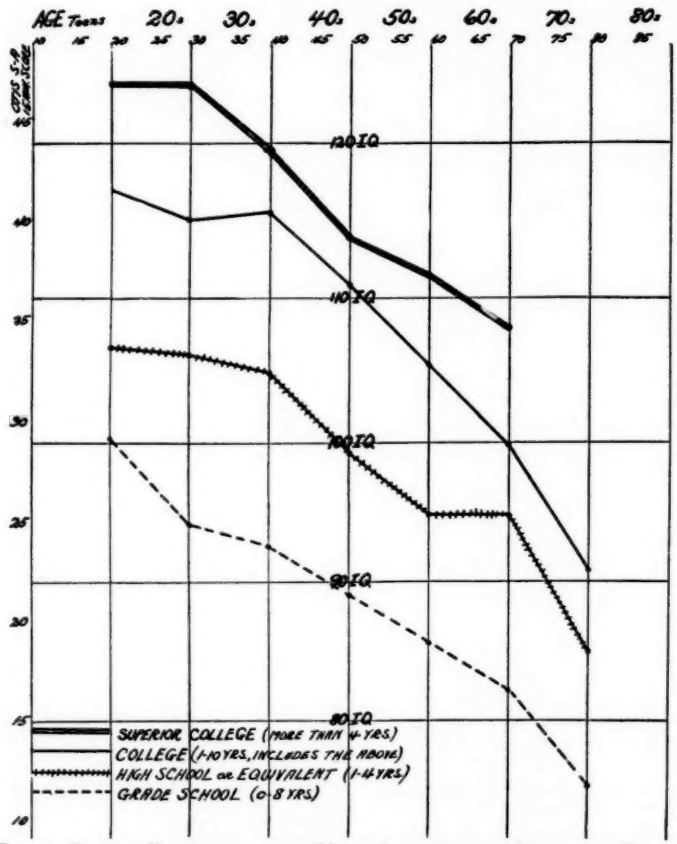

FIG. 5. DECADE DISTRIBUTIONS OF MEAN INTELLIGENCE SCORES FOR GROUPS REPRESENTING 4 EDUCATIONAL LEVELS
(Data from both cities combined)

for the same age period there were 289 persons who could be so classified. The percentage of *Ss* in each of the classes was as follows.

	City A	*City B*
Grade School	60%	27%
High School	26%	44%
College	14%	29%

This striking dissimilarity in the constitution of the two groups and the resultant divergence of the curve of one from that of the other suggested the desirability of analyzing both populations in terms of educational status.

In deriving mean scores for the three educational levels decade by decade the results from the two cities are combined. The means are presented in Tables XIX to XXI inclusive with the ratios giving the significance of the differences between successive means, and also the equivalent scores in terms of Otis mental age, IQ, percentile, and Army Alpha score. Table XXII gives the significance of the differences between the educational means decade by decade, and Table XXIII the correlations between age and score for each sex and the sexes combined in each educational level. Plotted curves for the three contrasted groups appear in Fig. 5. Here in addition to the college group as represented in the tables we have set off a superior (more than 4 yrs.) college population.

TABLE XIX

OTIS S-A INTELLIGENCE SCORES BY DECADES FOR GROUPS WITH GRADE SCHOOL (0-8 YR.) EDUCATION

Otis 15-min. S-A Scores					Equivalent Measures				
No. Cases	Decade	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
3	20s	29.3	6.4	.88	42.5	15·5	101	52	84
11	30s	24.9	10.8	.33	36.0	14·4	94	30	67
18	40s	23.7	6.8	1.10	35.0	14·2	93	28	64
41	50s	21.3	9.6	1.25	31.0	13·5	89	18	56
45	60s	18.9	8.0	1.47	27.0	12·8	85	10	41
36	70s	16.5	6.4	1.64	24.0	12·2	82	6	32
12	80s	11.7	9.6		17.0	10·10	75	2	19

TABLE XX

OTIS S-A INTELLIGENCE SCORES BY DECADES FOR GROUPS WITH HIGH SCHOOL (9-12 YEARS) EDUCATION

Otis 15-min. S-A Scores					Equivalent Measures				
No. Cases	Decade	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
24	20s	33.7	6.4	0.22	48.7	16·4	107	72	103
36	30s	33.3	8.0	0.39	48.3	16·3	106	71	101
29	40s	32.5	8.8	2.06	47.2	16·2	105	67	98
62	50s	28.5	8.8	1.80	41.5	15·2	100	48	81
51	60s	25.3	9.6	0	36.8	14·6	95	33	69
30	70s	25.3	8.0	2.27	36.8	14·6	95	33	69
6	80s	18.5	6.4	1.54	26.5	12·7	85	9	39
1	90s	14.5	0		21.0	11·7	79	4	26

It is strikingly evident, first of all, that the same tendency for increasing age to be associated with intelligence score decrement is a characteristic of each of the educational groups. In IQ points lost from the 'fifties' to the 'nineties' there is little to choose between the three, the people of grade school education lose 26 points, those with high school training 28, and those with college opportunity 27. The correlations suggest that there is a slightly stronger relationship between score and age the higher the educational class.

TABLE XXI

OTIS S-A INTELLIGENCE SCORES BY DECADES FOR GROUPS WITH COLLEGE (1-10 YR.) EDUCATION

Otis 15-min. S-A Scores					Equivalent Measures				
No. Cases	Decade	Mean	σ	D/σ diff.	Otis 30-min. score	Otis MA	Otis IQ	Otis Per-centile	Army Alpha
51	20s	41.7	6.4		59.5	17'9	118	93	142
				1.19					
39	30s	40.1	7.2		58.0	17'6	116	91	136
				−0.21					
37	40s	40.5	9.6		58.5	17'7	117	92	138
				1.80					
42	50s	36.9	7.6		53.0	16'11	111	82	117
				2.00					
35	60s	32.9	9.6		48.0	16'3	106	70	100
				1.47					
19	70s	28.9	9.6		42.0	15'4	100	50	84
				2.91					
1	80s	22.5	0		33.0	13'10	91	22	60

TABLE XXII

INTELLIGENCE SCORES BY DECADES AT THREE EDUCATIONAL LEVELS WITH THE SIGNIFICANCE OF THE DIFFERENCES BETWEEN THEM

(Both Cities, Both Sexes)

Decade		No. cases	Mean	σ	D/σ diff.
20s	Sup. Col. & Col.	51	41.7	.9	
					5.0
	H.S. & Norm. & Bus.	24	33.7	1.3	1.0 } 3.2
	Gr. sch.	3	29.3	3.8	
30s	Sup. Col. & Col.	39	40.1	1.0	
					4.2
	H.S. & Norm. & Bus.	36	33.3	1.3	2.3 } 4.3
	Gr. sch.	11	24.9	3.3	
40s	Sup. Col. & Col.	37	40.5	1.6	
					3.5
	H.S. & Norm. & Bus.	29	32.5	1.6	3.8 } 7.3
	Gr. sch.	18	23.7	1.6	
50s	Sup. Col. & Col.	42	36.9	1.2	
					5.2
	H.S. & Norm. & Bus.	62	28.5	1.1	4.0 } 8.2
	Gr. sch.	41	21.3	1.5	
60s	Sup. Col. & Col.	35	32.9	1.6	
					3.6
	H.S. & Norm. & Bus.	51	25.3	1.4	3.6 } 7.0
	Gr. sch.	45	18.9	1.2	
70s	Sup. Col. & Col.	19	28.9	2.2	
					1.4
	H.S. & Norm. & Bus.	30	25.3	1.5	3.1 } 5.0
	Gr. sch.	36	16.5	1.1	
80s	Sup. Col. & Col.	1	22.5	0	
					1.5
	H.S. & Norm. & Bus.	6	18.5	2.6	1.8 } 4.0
	Gr. sch.	12	11.7	2.7	

TABLE XXIII

CORRELATIONS BETWEEN AGE AND INTELLIGENCE SCORE IN THE AGE RANGE 20 TO 95 YR. AT THREE EDUCATIONAL LEVELS (Cities A and B Combined)

Educational Level	Sex	No. Cases	Product-moment r
Grade School	M	74	−.401 ± .065
(0-8 Grades)	F	92	−.361 ± .061
	M & F	166	−.394 ± .045
High School	M	64	−.413 ± .070
(9-12 Grades)	F	175	−.509 ± .038
and N. and B.	M & F	239	−.403 ± .037
College	M	121	−.436 ± .050
(1-10 years)	F	103	−.544 ± .047
and S. Coll.	M & F	224	−.439 ± .036

The decrement from decade to decade within the three educational levels is not large. No one of the differences is statistically significant for our comparatively small groups. But the fall is almost regularly continued whatever the degree of education and the result is a loss of 4 to 5 mental age years during the period of maturity.

The striking feature of the present comparison is the practical advantage of the person of greater education and higher initial IQ over the person of lesser education and poorer initial mental equipment. The individual who starts with the ability that measures during the period of growth at or near the average falls to the 6th intelligence score percentile for adults by the time he reaches age 75. The person who starts near the upper quartile falls during the same period to the low average score indicated by the 33rd percentile. The person whose rating during the period of growth registers at the 93rd percentile scores even in the eighth decade at the 50th intelligence score percentile which is the average for adults in general and an average based in large part on groups in the early maturity age range. Individuals having at this later period in life an intelligence scoring ability that is average or above for active adults and combined with it a great wealth of experience, easily maintain their eminence and leadership in the affairs of men and may successfully continue their business and professional activities to very advanced age. Such people are honored and revered, and exemplify human life at its acme of effectiveness and fulfillment.

Finally, in Table XXIV, the percentages by decade and educational level are given for the seven brightness classes. The correspondence between degree of education and degree of brightness is striking, and perhaps we might say encouraging. Class A numbering 117 persons shows no one who did not go beyond the grade school, and a total of 41% of those with some college training fall in this class. Class C registers only 13 people with college records as against 59 for high school and 50 for the grades. The D and E classes, as anyone might have predicted beforehand, were not wholly devoid of 'college people;' however they are few, 4 as against 60. But age shows its influence also and the fall from the higher to the lower classes persists regularly in a diagonal line across the table from decade to decade, whatever the extent of educational training and advantage.

TABLE XXIV

BRIGHTNESS CLASSES BY DECADES AT THREE EDUCATIONAL LEVELS
(Cities A and B; both sexes)

		Decade								Total % all decades combined	Total no. cases all decades combined
		20s	30s	40s	50s	60s	70s	80s	90s		
A	Gr. sch.									0	
	HS & NB	16.6	20.0	24.1	4.9	7.8	3.2			10.9	26
	S. Col. & Col.	58.0	47.4	60.5	31.0	23.5				41.0	91
B	Gr. sch.	33.3	25.0		9.8	2.2				5.4	9
	HS & NB	33.3	34.3	10.4	18.0	9.8	19.3			18.9	45
	S. Col. & Col.	38.0	34.2	10.5	35.7	29.4	36.8			30.6	68
C+	Gr. sch.	33.3	16.6	33.3	24.4	20.0	5.7	8.3		18.7	31
	HS & NB	33.3	31.4	44.8	32.8	29.4	25.8	16.6		31.9	76
	S. Col. & Col.	4.0	15.8	23.7	26.2	23.5	26.3			18.5	41
C	Gr. sch.	33.3	33.3	44.4	29.3	24.5	34.3	16.6		30.1	50
	HS & NB	16.6	11.4	13.8	31.2	29.4	32.3	50.0		24.8	59
	S. Col. & Col.		2.6		4.8	14.7	21.0	100.0		5.9	13
C−	Gr. sch.		8.3	22.2	12.2	22.2	25.7	8.3		18.1	30
	HS & NB		2.9	6.9	9.8	11.8	6.5		100.0	7.6	18
	S. Col. & Col.			5.3	2.4	5.9				2.3	5
D	Gr. sch.				7.3	11.1	11.4	16.6		8.4	14
	HS & NB				3.3	5.9	6.5	16.6		3.4	8
	S. Col. & Col.						10.5			.9	2
E	Gr. sch.		16.6		17.1	20.0	22.9	50.0		19.3	32
	HS & NB					5.9	6.5	16.6		2.5	6
	S. Col. & Col.					2.9	5.3			.9	2
Total %	Gr. sch.	99.9	99.8	99.9	100.1	100.0	100.0	99.8		100.0	
	HS & NB	99.8	100.0	100.0	100.0	100.0	100.1	99.8	100.0	100.0	
	S. Col. & Col.	100.0	100.0	100.0	100.1	99.9	99.9	100.0		100.1	
Total no. cases	Gr. sch.	3	12	18	41	45	35	12			166
	HS & NB	24	35	29	61	51	31	6	1		238
	S. Col. & Col.	50	38	38	42	34	19	1			222

Discussion

The results presented in this paper we have qualified because of the size of the samples and the character of the populations studied, but we have no reason to surmise that the findings would be significantly different if the investigation were to be repeated on a larger scale in several communities representing different social and economic strata. We have been careful to describe and discuss our data as measures of intelligence in the form of scores on the 15-min. Otis S-A Higher Examination. While not inclined to weave a halo about this test we are nevertheless bound to express some confidence in it as an indicator. For the practical purpose in hand we know of no better present-day instrument. The examination was administered with great care as to procedure and detail and under ideal conditions. It bore the caption, "Good-Judgment Question Series," with no mention of "intelligence" or "mental ability." It was not forbidding, in fact the contrary, as proven by the record that only 3 people out of 856 for any subjective reason balked at trying. A positive score could be made by practically everybody from 7 years of age to the oldest persons who could leave their homes, while at the same time only 4 members of the rather large group examined were able to finish, having tried all 60 items offered. The trustworthiness of the 15-min. test score compared to a similar test of 20- or 30-min. duration has been amply demonstrated, as well as in comparison with much longer examinations.

We examined intelligence by what is called a speed test in distinction to a power test. The circumstances of the experimental program seemed to demand this technique. Of course it may be objected that aging is itself a biological slowing down and that effective intelligence in the mature is not necessarily a thing that, to work well, must function at whip-cracker speed.

This common observation on the decreasing speed of the old is doubtless a popular way of recognizing and naming the progressive decrement discussed in this paper. While the speed test may seem to some unfair to the old, we can not close our eyes to the strong probability indicated by many well conducted studies, which we need not discuss now, that speed and power in mental operations are closely related and at base may well be dependent on the same conditions.

The factor of disuse or practice we have not been able to analyze or equate in the successive decade groups. It seems probable that the test items are not greatly at fault in this respect, as evidenced by the very wide applicability of the test. Subjects did not complain of inability to understand the wording of the items nor did they claim that the questions were unfamiliar in form or substance. Naturally some of those in the 'teens' were more familiar with being tested but there was probably little difference in this respect for all adults over 30 years of age.

Taking all of these things into consideration we see no conditions requiring that a special discount be levied against our findings nor, on the other hand, should they be considered beyond criticism. To use a phrase from Claude Bernard, "The investigator should have a robust faith and yet not believe." We have tried to do both of these things in the present study. But it is our opinion that anyone who works with a large or moderately large body of data, representative of normal human intelligence throughout the life-span, is certain to be impressed with the general regularity of the results and the characteristic course taken by the mean values for the successive ages.[19]

Summary

(1) The correlation of intelligence scores with chronological age during childhood and youth is well-known and has often been demonstrated. Hypothetical curves for the course of intelligence in later life have been drawn, usually from studies on more or less atypical groups and with relatively few data for ages beyond 50 years. The correlation coefficients previously reported have ranged from −0.011 to −0.35.

(2) The relation between age and intelligence throughout the life span was investigated in the present study by securing intelligence scores on samplings, (from two fair sized American towns), for every decade and semi-decade from 7 to 92 years of age.

(3) All the intelligence tests for both children and adults, males and females, were made by means of a large type edition of the first 60 items of the Otis Self-Administering Test of Intelligence, Higher Examination, Form A, given as a 15-min. test. Such a 15-min. test

[19]We cannot close this brief presentation of the tabular analysis of these data on later maturity without acknowledging the financial help of the Carnegie Corporation of New York and the faithful work of three assistants: Mrs. J. S. Carroll, Mrs. W. L. Iversen, and Mrs. I. J. Cooper, all of whom conducted examinations.

has been found to correlate $+0.86 \pm .02$, with scores on a similar 30-min. test in the limited range of superior ability represented by 121 university students. The test was administered individually under ideal conditions. Only 3 people out of a total of 856 for any reason objected to taking the examination, which passed under the name of "Good-Judgment Question Series." Only 4 people of the entire group were able to complete the test in the 15-min. period.

(4) For the age span 7 to 17 years inclusive the coefficient found between age and score was $+0.80 \pm .023$. The age group ranging from 15 yr. 0 mo. to 16 yr. 11 mo. registered a mental age of 17·4, and the next 2-yr. group, which represents the peak of ability in these findings, exceeds this level by only one month, (17·5).

(5) From the high point in the intelligence score curve represented at about 18 yrs. of age the trend is at first almost level, then gradually declines, dropping 15 or 16 mental age months by the chronological age of 50 years. In this period covered by the 3rd to the 5th decade the decline in Otis IQ points is from 115 to 104.5 and the correlation coefficient indicating the relation of score to age is -0.283 for males and females combined.

(6) The drop in intelligence score from age 18 to age 85 (City B) amounted to 62% of the original high score and in mental age months to a decline of 5 yr. and 10 mo. The correlation between age and score from the age of 20 to 95 years is approximately $-0.50 \pm .02$ and the difference between the average scores in the 'twenties' and in the 'eighties' (City B) is 9.9 σ diff.

(7) Distribution of the Otis 15-min. scores in terms of the Army brightness classification, A to E, shows that the Classes A and B progressively decrease with increasing age, and disappear altogether in both populations in the 9th decade. Groups C−, D, and E, practically non-existent in the earliest adulthood in our population, include altogether 35% in the 8th and 55% in the 9th decade.

(8) The downward trend of intelligence scores as age increases is shown to be definitely a characteristic of both sexes. In 3 or 4 comparisons out of 5 the men demonstrate somewhat better ability to score on the test, but this superiority is not regular throughout the life span and may be attributable chiefly to errors in sampling.

(9) The two cities studied exhibited considerable average difference in educational level, but the curves for decline of score with age were found to follow parallel courses. When classified according to educational level all groups exhibited the same tendency for in-

creasing age to be associated with intelligence score decrement. From 50 to 90 years people of grade school education lost 26 Otis IQ points, those with high school training 28, those with college opportunities 27 points.

(10) The correspondence between the degree of education and the degree of brightness was prominent. There was a slightly stronger relationship between score and age in the highest educational class, but the great advantage of the individual who starts with high initial ability is evident. The person who registers, in youth, at the 93rd percentile score, according to the rate of decline found, would be in the 50th intelligence score percentile for average adults on reaching the 8th decade of life.

THE RÔLE OF FORM IN PERCEPTION

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The work of the configurational psychologists has brought the question of form into the focus of interest once again. In much of the experimentation on apparent movement form has been found to play a determining part and this has led to generalizations concerning the rôle of form in perception and behavior. A number of terms, supposedly descriptive of forms, have crept into the literature and have been accorded the status of 'laws.' Such terms as *Prägnanz*, closure, and good or bad have been used to describe, explain and predict how complexes behave, but their meanings and the precise limits of their operation have not been clarified with usage.[1] While the suggestiveness and fruitfulness of new terms are not denied it is customary in science to demand that words shall have precise, univocal meanings. Moreover it is time that the concepts underlying broad generalizations forming a new system of psychology be submitted to experimental verification. If the form of a stimulus complex influences what is seen or reacted to, then we should be able to find quantitative evidence for such influences and if some forms are 'better' than others their superiority should emerge under changing conditions in which the only variable is the form factor. This study was therefore undertaken to determine if form plays a decisive rôle in perception by measuring the lower thresholds for light when, all other factors constant, the form of the stimuli was changed. A further aim was to discover if some forms, notably the circle which has been proclaimed the perfect figure by the configurationists, were better than others according to some quantitative criterion.

If the liminal value changes for different forms, then form is certainly of importance in many ways: in the measurement of the light sense, in many laboratory experiments in vision or in other modalities, and in the systematic position to be accorded form as such. But if

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[1]For a review and criticism of Gestalt psychology the reader is referred to H. Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-216. W. D. Turner (Intra-serial effects with lifted weights, *ibid.*, 43, 1931, 1-25) has criticized the concept of Prägnanz as unclear.

differences in form do not result in measurable differences in observation then the generalization, often made, that form is of universal and primary importance in perception and other modes of behavior will have to be retracted or modified in favor of a truer statement of its actual efficacy.[2]

Historical

The problem of form *qua* form, as well as the general problems raised by *Gestalttheorie*, is by no means a new one in psychology. Perhaps one of the first to emphasize and recognize the rôle of form in perception was Ernst Mach who wrote as early as 1886: "In examining two figures which are alike but differently colored we recognize their sameness of form at the first glance, in spite of the differences of color-sensation. The sight-perceptions, therefore, must contain some like sensation components. These are the space-sensations which are the same in the two cases."[3] He also points out that two squares, one on a side, the other on a corner, could never be recognized as the same type of figure without "mechanical and intellectual operations in addition to the sensory processes in the perception." Husserl,[4] writing in 1891, expressed himself in terms that sound strangely modern: "Wir erfassen ihre Configuration (*i.e.* eine sinnliche Menge) ganz wie einer Qualität in einem Blick." He later declares that the quality belonging to a group as such is its figural factor and, anticipating Köhler, while admitting the importance of the relations among the members of the group, denies that they are solely responsible for the unified quality of the whole.

It remained for Von Ehrenfels to give the facts such a twist that the attention of psychologists was gained and a large literature devoted to the subject of the *Gestaltqualität*. The controversy over the status of the form-quality involved almost every psychologist of the time and contains almost every argument, pro and con, advanced today with regard to the importance of the concept of *Gestalt*. While on the theoretical side it drew Meinong, Schumann, Cornelius, Höfler, Bentley, and even Stumpf, through his student Gelb, into the fray, it gave rise to some beautiful experimentation as found in the work of Schumann, Benussi, Seifert and a host of others.[5] It is impossible to review this voluminous literature at this point, so mention only will be made of the original statement by Von Ehrenfels and the contributions of Schumann in order to show what general problems were introduced through a consideration of the form factor in perception.

In asking his question which was to be debated for the next forty years, Von Ehrenfels said: "An individual *S* comprehends a melody which consists of n tones, and, on the other hand, n individuals hear each one of the n tones. Does *S*

[2] The configurationists' emphasis upon form will be found in practically all of their publications. It recurs again and again. Less definite synonyms for *Gestalt* which are often used are: unity, whole, pattern, complex, structure, and *Bauplan* (Köhler). This is not a matter of different translations of the same word, for the German literature abounds in numerous terms and statements referring to factors supposed to have a part in the total *Gestalt*.

[3] E. Mach, *Analysis of Sensations*, 1897, 137.

[4] E. G. Husserl, *Philosophie der Arithmetik*, 1891, ch. 11, 22.

[5] A full discussion will be found in I. M. Bentley, Psychology of mental arrangement, this Journal, 13, 1902, 268 ff. From 1902 on the literature is covered by Helson's review, *op. cit.*

comprehend more than the other n individuals taken together? There must be something more in the form than a sum of elements. In the perception of spatial figures, is there more in a space form than n determinations of position, or the lines plus relations making up the figure?"[6] He then advances arguments which demonstrate the difficulties of atomistic theories designed to explain *Gestaltqualitäten* as sums of sensations: "for if a space-form is nothing but a sum of local determinations, various space-forms should be the more alike the more their single elements are alike. But since the local determinations depend upon the state of the visual field, then they should be more alike the nearer they are placed together. On this hypothesis it is impossible to see how two rectangles remain exactly alike no matter how far apart they are separated from each other and why two rectangles near each other are no more alike than two separated from each other."[7] Form depends therefore upon something other than similarity of elements and the relation of similarity depends upon more than a likeness of parts. The two criteria determining form-quality are then given: it is over and above the elements in the configuration (a new positive ideational content in consciousness) and it is transposable from one set of elements to another, hence independent of them.

It can be easily seen from this statement of the problem raised by Von Ehrenfels and elaborated by many others who discussed it after him that we really have a number and variety of issues, some of which are independent from our concern in this paper. Before passing to the work of Schumann which relates directly to our narrower problem of form, suffice it to say here that the reaction to Von Ehrenfels' "positive (and new) ideational content" was negative! While most psychologists were ready to admit the reality of the form-quality as a perceived datum, many refused to grant that a new sensation had been discovered or that radically different concepts were necessary to explain it. The consensus of opinion seemed to be that "parts in relation" would suffice to take care of the facts stressed by von Ehrenfels although Husserl had examined this type of explanation and demonstrated its weakness.

Schumann, while refusing to grant that there was anything radically new in Von Ehrenfels' findings, proceeded to investigate the problem of form in perception and published between 1900 and 1904 a series of papers on the problem that should, and probably will, be classical. Anyone reading these papers cannot doubt for a moment that in them are to be found the really direct precursors of Wertheimer, Köhler, and Koffka; in them will be found observations, explanations and glimmerings that might well be mistaken for material published in the *Forschung* of 1930. Schumann, moreover, *consciously* said these things. To find half-baked, unconscious, semi-poetical anticipations of present trends in earlier writers is to give credit where none is really due. But in the case of Schumann no one can doubt that he realized the importance of the factors stressed by Von Ehrenfels before him and the *Gestalttheoretiker* afterwards. Moreover, Schumann was careful to point out, what later writers have not been so careful to do, that the concept of form is ambiguous and that it may become a mere name for

[6] C. von Ehrenfels, Über Gestaltqualitäten, *Vierteljahrsschrift f. wiss. Phil.*, 14, 1890, 249 ff.

[7] *Idem.*

a phenomenon and not an explanation.[8] To attempt to summarize Schumann's contributions would be beyond the scope of this paper and tantamount to repeating much that passes for *Gestaltpsychologie* today. We mention only a few of the points stressed by Schumann: equal distances between members help form groups; surfaces between groups appear larger than equal surfaces between members of groups; nearness helps in the formation of groups; contours of unities tend to stand out in perception; incomplete figures tend to be seen as complete; ambiguous figures may be seen as 'good' figures now this way, now that; certain figures display properties characteristic of them as such; some parts of a figure bring out certain properties while others bring out still other properties; vertical symmetry is conducive to connectedness; *probably the main locus of the properties of figures is to be sought in central factors.* Schumann rejects Von Ehrenfels' new element, preferring to explain his findings by asserting that figures possess characteristic properties belonging to them as such. Nor is his discussion limited to vision: analogies and suggestions abound in his articles for memory, the affects, and the various sense modalities.[9]

From Schumann to the work of the modern school of *Gestalt* is but a very small step, if any at all. The work of this school has been summarized and criticized by Helson[10] so that it is only necessary to point out here that much of the experimental work of the configurationists and much of their theorizing is directed toward showing the importance of the form factor in perception and behavior. While there is ample evidence that form plays a part in the perception of movement, in binocular and stereoscopic vision, after-images, figure-ground phenomena and in many other places, it appears from a survey of the literature that the concept, with the concept of the *Gestalt*, suffers from ambiguity. In his articles on *Gestalt* Helson pointed out that the concept of the *Gestalt* suffered from manifold ambiguities and unclearnesses in meaning the following: (1) the form of an apprehended whole; (2) some factor within a group which dominated the whole; (3) the totality of conditions determining a perception, memory, or behavior pattern; (4) physical structures; (5) physiological structures; (6) biological structures; (7) logical structure; (8) psychological structure; (9) purpose; (10) necessary and sufficient conditions, etc.[11] While Köhler in his recent book has somewhat dispelled the haze surrounding the concept of the *Gestalt* by defining it as any "segregated whole or unit" and has elucidated the relation between the *Gestalt* and form by telling us that "in some respects form is the most important property which a whole may have,"[12] nevertheless, as Schumann pointed out, the word "unity" is itself open to criticism. Furthermore, the assertion that

[8]F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen, *Zsch. f. Psychol.*, 23, 1900, 1-32.

[9]*Ibid.*, 29.

[10]*Op. cit.*

[11]*Op. cit.* The ambiguity of the concept of *Gestalt* which was pointed out by the senior author on the basis of his survey of the configurational experimental literature in the articles referred to above reappeared when univocal criteria defining "goodness" of figure were sought in the present study. It may be of interest to note that the criticisms levelled against the configurationists by the senior author in 1926 on theoretical considerations are amply borne out by this experimental study.

[12]W. Köhler, *Gestalt Psychology*, 1929, 219.

"the most primitive phenomena are figural"[13] and statements to the same effect which assert that form is present in the earliest and simplest experiences need to be tested as generalizations by an appeal to experiment in as wide a variety of problems as possible. So, too, must the various characterizations of forms as good, bad, *prägnant*, and the like, be submitted to observational and quantitative tests to determine (1) whether they have precise, verifiable and unequivocal meanings; and (2) whether they denote the constitutive or important determiners of perception.

Some work has been done directly upon the question of the priority of form *versus* content and the results do not substantiate Koffka's claim given above. The work with patients suffering from brain lesions resulting in disturbances of visual function has shown that form perceptions may be lost while brightness and color vision remain. This fact has been reported not only by Poppelreuter and others, but substantiated by Gelb and Goldstein[14] who belong to the so-called school of *Gestaltpsychologie*, yet nothing has been made of it, in spite of its crucial importance for the claims of configurationists who assert that form is primary and prior in perception.

Several investigations carried out in this country both on the skin and in vision agree that form is not primary as such. Zigler and Northrup, in their 1926 study, report that, in spite of the fact that their *Os* were instructed to look for forms when geometrical figures were applied to the volar surface of the left forearm, "all *Os* were often unable to perceive any shape at all, not even enough to warrant a guess." "A definite and complete form was the exception rather than the rule."[15] The *Os* felt pressures as such before forms were felt. They conclude that "tactual form is not so definitely and unfailingly apprehended as tactual pressure. . . . Tactual pressure is invariably perceived, while tactual form is not definitely given in a considerable percentage of cases, and is not apprehended at all in an appreciable number of instances."[15]

Again, in 1927, Zigler and Barrett found that form does not appear first in tactual perception.[16] They describe three stages in perception: (1) an initial or preliminary stage in which a touch of indeterminate shape is felt; (2) an intermediate stage in which some prominent feature suggested a tentative but unsatisfactory shape; (3) the stage of definitely perceived form with greater or less clarity.

When we come to vision we discover that forms do not appear first in perception any more than they do on the skin. Zigler, Cook, Miller, and Wemple in an investigation of the perception of form in peripheral vision report:[17] (1) there is no form in the periphery of vision, only *figureless-background*; (2) as the stimulus passes toward the center of vision, formless figures are seen; (3) then comes a form resembling the figure actually used as stimulus; (4) and finally, clear form.

[13]K. Koffka, *The Growth of the Mind*, 1925, 132.

[14]W. Poppelreuter, Zur Psychologie der optischen Wahrnehmung, *Zsch f. d. ges. Neurol. u. Psychiat.* 83, 1923, 26 ff.; A. Gelb and K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle*, 1920, 36 ff.

[15]M. J. Zigler and K. M. Northup, The tactual perception of form, this JOURNAL, 37, 1926, 393 f.

[16]M. J. Zigler and R. Barrett, A further contribution to the tactual perception of form, *J. Exper. Psychol.*, 10, 1927, 186.

[17]M. J. Zigler, B. Cook, D. Miller, and L. Wemple, The perception of form in peripheral vision, this JOURNAL, 42, 1930, 246-259.

Nor do they find any differences among forms in the place where they change from formless to clear—all change at about the same region of the retina. Form as such not only fails to appear, but exerts no effect upon what is seen and where it is seen, otherwise there would be a difference for different forms. These authors tell us that "No type of figure manifests a consistent advantage over others in the matter of being perceptible farther afield." Exactly similar results have been stated by Kleitman and Blier.[18]

Problem

The present study was undertaken in an endeavor to determine if form as such exerted a constitutive effect upon the lower threshold of vision and, if such effects were found, what forms were 'good,' 'bad,' or 'indifferent.' Furthermore, we hoped to be able to set up criteria for the various types of forms, believing that those which emerged in the various categories could be classified by some quantitative or qualitative norm coming from the experiments themselves. In all of these our expectations were disappointed as the following pages will show.

Experimental Procedure

Our experiments consisted in determining the lower limens for light, just noticeable form, and certain form for 6 different figures, all of the same area. The figures used were: an isosceles triangle, standing on its base, 44.72 × 44.72 × 63.24 mm.; a rectangle, on its longer side, 50 × 20 mm.; a circle, diameter of 35.6 mm.; a semicircle, resting on its straight side, diameter of 50 mm.; an angle which consisted of a square, 35 × 35 mm. from which a smaller square 15 × 15 mm. had been taken, leaving an angle having an area of 1,000 sq. mm. like the other figures and presented like an inverted V; and finally, a square, 31.62 mm. on a side.

The figures were cut out of black cardboard and placed in front of the milk glass screen forming the front surface of the Nagel adaptometer.[19] The experiments were carried on after 30 min. of dark adaptation in a dark room. Before going into the dark room all *Os* gazed at a white wall illuminated by a lamp of small wattage in order to start dark adaptation from approximately the same level of light adaptation. Both periods of adaptation insured a fairly stable state for the observations to determine limens.[20] A 1-min. rest intervened between observations. A small red cross was so reflected from a mirror that it appeared to come from about 3 cm. above the stimulus figure. The purpose of the red fixation light was to insure foveal vision both because forms are best seen in the fovea and because it is necessary to work only on one part of the retina. The red of our fixation light helped insure the latter because red is a photopic color, *i.e.* its brightness is no greater for the rest of the retina than for

[18] Quoted by Zigler, Cook, Miller, and Wemple, *op. cit.*, 247.

[19] For a description of the apparatus see W. Lohmann, Über Helladaptation, *Zsch. f. Sinnesphysiol.*, 41, 1907, 304 ff.

[20] This is the time usually taken to insure equilibrium, although some experimenters believe 15-20 min. sufficient.

the fovea and hence there is no tendency to turn the eye away from foveal vision to a place of greater sensitivity. Furthermore, in order to insure foveal vision the *Os* sat at a distance of 275 cm. from the instrument which made the angle subtended by both fixation and stimulus lights not greater than one degree—viz. 57′. This angle falls well within the foveal region which is usually said to be 2° 30′.

With 3 *Os*, limens for 'just perceived light,' 'just perceived form,' and 'certain form' were determined separately, while the fourth *O* was instructed to increase the intensity of the light by turning a handle until she first saw light when she was to report its form if she could and the degree of certainty. If she was wrong or uncertain she was instructed to keep turning until she was right in her report and certain of it. We thus obtained three sets of limens for each *O* on each figure: the limen for just noticeable light—since light was practically always seen before any trace of form was visible; the limen for just noticeable form—whether or not the form named was correct; and the limen for certain form—when the form exposed was correctly named and the *O* felt sure of his report. We have called these values respectively the Light Limen; Form 1; and Form 2.

By dividing the values given in our tables into the constant 0.0144 they can be translated into foot-candles of illumination or apparent foot-candles. As they stand, our figures represent measures of sensitivity and hence the actual limens vary inversely with them.

Only the ascending series in the Method of Limits was finally used in our computations for the following reasons: (1) at its best the lower limen for light is subject to large standard deviations when measured by a fine instrument and the inclusion of ascending and descending values would have given such large deviations that we feared differences among forms might be masked; (2) had we used the descending series throughout our experiments, we would not have been able to determine whether forms are seen coincidently with light, before, or afterwards—one of the points our study was undertaken to settle; (3) knowledge of the form prevented the *Os* from reporting when they actually disappeared in the descending series and thus contributed to the large deviations. In order therefore, to keep our *Os* in ignorance of the stimulus at each observation and to reduce the size of the deviations, which is considerable in any case, we presented our stimuli in chance order, used only the ascending series in the Method of Limits, and began with a different setting of the handle which the *O* turned to increase the intensity of the stimulus light.

Results

(*1*) *Form as a primary datum.* The first and, perhaps, most significant result appears from a comparison of the limens for just noticeable light, Form 1 and Form 2, in Tables I, III, and V. In Table I are given the values of the light limens for each *O* on each figure, as well as the average value for all *Os* on each figure. Tables III and V give individual and group values for Form 1 and Form 2. We find a tremendous difference in the limens for the three stages of perception as the following averages, for all *Os* on all figures, for the three cases show: Light = 647.11; Form 1 = 44.76; Form 2 = 26.03.

TABLE I

VALUES OF 'LIGHT' LIMENS AND σ FOR EVERY *O* ON EVERY FIGURE

		Triangle	Square	Rectangle	Circle	Semicircle	Angle
D	L	33.38	32.39	38.75	40.00	34.10	39.50
	σ	19.69	19.43	18.21	17.60	13.98	21.73
H	L	2003.20	1852.60	1916.23	1943.40	1658.70	1972.84
	σ	781.25	648.41	638.74	621.89	727.83	789.13
M	L	401.50	385.92	425.51	377.19	351.24	410.75
	σ	128.48	100.34	127.72	125.73	122.93	127.33
Z	L	271.26	280.00	264.47	294.58	219.06	284.23
	σ	100.37	117.60	111.08	94.27	74.48	127.90
Av. L		677.35	637.72	661.24	663.79	565.77	676.83
σ dis.		873.00	787.00	807.00	815.00	741.00	861.00
σ m		61.74	55.66	57.07	57.64	52.40	60.89

TABLE II

SHOWING FOR THE VARIOUS PAIRS OF LIGHT LIMENS THE DIFFERENCE (D), THE STANDARD DEVIATION OF THE DIFFERENCE (σ DIFF.), AND THE RATIO (D/σ DIFF.) BETWEEN THOSE VALUES

Figures		Tri.	Sq.	Rect.	Circle	Semi-circle
	D	39.61				
Square	σ diff.	83.13				
	D/σ diff.	0.47				
	D	16.09	23.52			
Rect.	σ diff.	84.07	79.72			
	D/σ diff.	0.19	0.29			
	D	13.54	26.07	2.55		
Circle	σ diff.	84.46	80.13	81.11		
	D/σ diff.	0.16	0.32	0.03		
	D	111.56	71.95	95.47	98.02	
Semi-c.	σ diff.	80.98	76.45	77.48	77.90	
	D/σ diff.	1.37	0.94	1.23	1.25	
	D	00.50	39.11	15.59	13.04	111.06
Angle	σ diff.	86.72	82.50	83.46	83.84	80.33
	D/σ diff.	0.00	0.47	0.18	0.15	1.38

TABLE III

VALUES OF FORM 1 LIMENS AND σ FOR EVERY O ON EVERY FIGURE

		Triangle	Square	Rectangle	Circle	Semicircle	Angle
D	L	6.57	6.18	7.50	5.53	4.42	4.20
	σ	1.77	1.79	1.95	1.60	1.37	1.93
H	L	135.63	142.14	207.66	147.96	90.42	74.40
	σ	86.08	63.96	97.60	76.94	46.11	37.94
M	L	15.36	11.54	14.21	11.20	7.08	9.30
	σ	8.22	6.23	8.67	6.27	3.11	4.84
Z	L	30.42	27.70	33.00	30.00	25.33	26.56
	σ	16.73	14.40	15.50	12.30	14.44	14.61
Av. L.		46.99	46.89	65.59	48.67	31.81	28.61
σ dis.		68.04	64.59	96.65	69.97	42.35	34.45
σ m		4.81	4.56	6.83	4.95	2.99	2.42

TABLE IV

SHOWING FOR THE VARIOUS PAIRS OF FORM 1 LIMENS THE DIFFERENCE (D), THE STANDARD DEVIATION OF THE DIFFERENCE (σ DIFF.), AND THE RATIO (D/σ DIFF.) BETWEEN THOSE VALUES

Figures		Triangle	Square	Rectangle	Circle	Semi-circle
	D	0.10				
Square	σ diff.	6.63				
	D/σ diff.	0.001				
	D	18.60	18.70			
Rect.	σ diff.	8.35	8.21			
	D/σ diff.	2.22	2.27			
	D	1.68	1.78	16.92		
Circle	σ diff.	6.90	6.73	8.44		
	D/σ diff.	0.24	0.26	2.00		
	D	15.18	15.08	33.78	16.86	
Semi-c.	σ diff.	5.66	5.45	7.46	5.78	
	D/σ diff.	2.68	2.76	4.52	2.91	
	D	18.38	18.28	36.98	20.06	3.20
Angle	σ diff.	5.38	5.15	7.25	5.51	3.85
	D/σ diff.	3.41	3.54	5.10	3.64	0.83

These differences mean that the sensitivity of our *O*s is nearly 25 times as great to 'just noticeable light' as to 'certain form;' nearly 15 times as great to light as to 'just perceived form' wherein the forms are usually reported incorrectly; and nearly twice as great to 'just perceived form' (unclear, hazy, guessed-at forms) as to 'certain forms.' The lowest limen is for light without form, the highest for clearly perceived forms, with the uncertain stage between the other two extremes. It appears that far from being primary in perception, form perception is the result of mechanisms at a much higher level of integration than those responsible for the perception of formless light. Our results substantiate the conclusions reached by various other workers and are in direct opposition to the assertion that form as such is present at the lowest levels of perception or that every perception must necessarily be of the figure-ground variety.

(*2*) *Light limens.* It might be maintained that even though form is not perceived at the lower limit of vision that it is nevertheless effective below the limen in that it determines what is seen, or has some influence upon the threshold value. This assumption may be tested by determining if there are significant differences between the limens for the different figures.[21] This has been done in Tables II and VII, the former giving the differences, σ, and σ diff. for all *O*s as a group, the latter giving the same information for each *O* separately in order to show individual differences, if any, between *O*s. Whether we group our data or not, both tables fail to show any significant differences among figures in their limens for just noticeable light. One exception to this statement is found for two figures for one *O* (*Z*) who happens to give a significant difference between her limens for circle and semicircle but since this is only one case out of a possible 60 and disappears when we treat her among the other *O*s in a group, it may well be due to chance, especially since this *O* shows

[21]When the ratio of the difference between two measures to the σ diff. is 3 or more, we have considered the difference as significant, otherwise not. It is obvious that the chances of a significant difference are nearly as great when the value of this ratio is 2.99 as when the value reaches 3. Our reason for neglecting all ratios below 3, no matter how close they may be to 3, is that the line must be drawn somewhere and since 3 is the generally accepted value indicating significance, we have adopted it. A lower value would not affect our conclusions in any way even though more significant differences among figures would thereby appear. The reader must keep this fact in mind throughout the rest of our discussion because we have taken as our criterion of significance this statistical value. On the other hand, the absolute values speak for themselves and justify our conclusions. But when we come to compare figures with each other some standard or statistical norm is necessary before we can even guess as to whether a difference in absolute values is significant.

TABLE V

VALUES OF FORM 2 LIMENS AND σ FOR EVERY O ON EVERY FIGURE

		Triangle	Square	Rectangle	Circle	Semicircle	Angle
D	L	2.10	2.20	2.16	1.63	1.44	1.11
	σ	0.63	0.51	0.63	0.51	0.45	0.36
H	L	80.37	95.04	197.30	77.97	60.00	46.17
	σ	38.58	52.27	82.87	36.65	25.80	20.78
M	L	3.70	3.60	3.46	3.09	2.77	2.99
	σ	1.25	0.97	1.03	0.96	0.62	0.77
Z	L	6.09	6.26	7.80	5.90	6.28	5.50
	σ	2.56	2.50	3.27	1.71	2.01	1.43
Av. L		23.06	26.77	52.68	22.15	17.62	13.94
σ dis.		38.35	46.95	93.25	37.12	27.74	21.38
σ m		2.71	3.32	6.59	2.62	1.96	1.51

TABLE VI

SHOWING FOR THE VARIOUS PAIRS OF FORM 2 LIMENS THE DIFFERENCE (D), THE STANDARD DEVIATION OF THE DIFFERENCE (σ DIFF.), AND THE RATIO (D/σ DIFF.) BETWEEN THOSE VALUES

Figures		Triangle	Square	Rectangle	Circle	Semicircle
	D	3.71				
Square	σ diff.	4.28				
	D/σ diff.	0.86				
	D	29.62	25.92			
Rect.	σ diff.	7.13	7.38			
	D/σ diff.	4.15	3.51			
	D	.91	4.62	30.53		
Circle	σ diff.	3.78	4.23	7.09		
	D/σ diff.	0.24	1.09	4.30		
	D	5.44	9.15	35.06	4.53	
Semi-c.	σ diff.	3.34	3.86	6.80	3.27	
	D/σ diff.	1.62	2.37	5.15	1.38	
	D	9.12	12.83	38.74	8.21	3.68
Angle	σ diff.	3.10	3.65	6.76	3.02	2.47
	D/σ diff.	2.94	3.51	5.73	2.71	1.48

TABLE VII

SIGNIFICANT DIFFERENCES BETWEEN FIGURES FOR LIMENS OF LIGHT, FORM 1, AND FORM 2

		Triangle			Angle			Semicircle			Circle			Rectangle		
O		Light	F-1	F-2	Light	F-1	F-2	Light	F-1	F-2	Light	F-1	F-2	Light	F-1	F-2
	Angle	1.60	5.08	9.17												
	Semi-c.	.20	6.31	5.80	1.71	.62	3.99									
D	Circle	1.82	2.98	4.00	.12	3.26	5.79	2.05	4.58	1.93						
	Rect.	1.36	2.49	.47	.34	6.33	9.56	1.45	6.04	6.26	.53	5.17	4.49			
	Square	.03	1.08	.84	1.84	4.45	10.13	.49	4.92	6.90	2.07	1.86	5.12	1.60	3.41	.31
	Angle	.18	5.13	5.59												
	Semi-c.	2.21	2.99	3.16	1.98	1.83	2.87									
H	Circle	.42	.72	.31	.20	6.08	5.38	1.98	4.51	2.81						
	Rect.	.60	3.43	8.13	.38	7.01	11.55	1.78	7.16	10.94	.53	3.25	8.41			
	Square	1.91	.41	1.61	1.56	5.95	6.80	.64	4.34	4.65	2.07	.40	1.91	1.41	4.00	6.14
	Angle	.61	4.47	3.59												
	Semi-c.	1.76	7.48	14.06	2.33	2.68	1.58									
M	Circle	.73	2.74	2.71	1.34	1.70	.58	1.04	4.58	2.04						
	Rect.	1.20	.66	1.04	.58	3.66	2.61	2.88	6.04	4.27	1.92	1.98	1.80			
	Square	.42	2.57	.42	1.08	1.99	3.49	1.49	4.92	5.31	.38	.26	2.54	1.75	1.78	.70
	Angle	..58	1.20	1.47												
	Semi-c.	2.40	1.59	.40	2.79	.42	2.26									
Z	Circle	1.20	.14	.22	.46	1.21	1.24	3.75	1.62	1.03						
	Rect.	.31	.89	2.86	.82	2.13	4.88	1.72	2.50	2.92	1.40	1.21	3.96			
	Square	.45	.86	.32	.12	.43	1.90	2.71	.80	.05	.62	.83	.85	.71	1.82	2.63

the fewest number of significant differences under all conditions. We are justified, on the basis of these results, in concluding that form is not a constitutive factor in the perception of light as such.

(*3*) *Form limens.* If we compare each of the 6 figures with every other there are 15 possible comparisons for the *O*s as a group, 60 comparisons if we treat the results separately for the 4 *O*s. This is true for Form 1 and Form 2 as well as for light perception. In Table VII the individual results have been listed for all three cases, the lack of significant differences for the light limens having been mentioned already. We can therefore turn our attention to the results for Form 1 and Form 2, discussing the former first. Treating the *O*s as a group, we see from Table IV that out of 15 possible significant differences, only 5 are actually significant as follows: triangle and angle; square and angle; rectangle and semicircle; rectangle and angle; and circle and angle. Four out of these 5 significant differences concern the angle and some other figure. It appears as if the low sensitivity value of the angle makes it a 'bad' figure compared with the others. But when we inspect the results for each *O*, as given in Table VII under Form 1, we find that other figures show as many significant differences as the angle, indeed more: the semi-circle shows 11 significant differences and the angle only 10 whereas in the group results the angle emerges as significantly different from several other figures. From the individual results the rectangle emerges with 11 significant differences, and since it appears significantly different from two other figures in the group results we might conclude that its high sensitivity value sets it apart as a 'good' figure from some of the others, even though less than half the figures are significantly different from it. Out of 60 possible significant differences in the individual results, we find a total of 26 really significant.[22] One *O*, (*Z*) shows no significant differences in the Form 1 limens! Before drawing any conclusions as to 'goodness' or 'badness' of forms we must see what still different criteria tell us.

Comparing the values given in Tables V, VI and VII for Form 2 (correctly perceived forms), we notice that out of 15 possible significant differences for the group results (Table VI) only 6 are signifi-

[22]It should be noted that the number of significant differences is just twice as great when we add the number for each figure as it is when we take the total number of all figures because in the former case each significant difference is counted twice, *e.g.* if rectangle and angle are significantly different so are angle and rectangle for both will be counted when we treat the figures individually, although there is really only one difference involved.

cantly different. These differences concern the following figures: triangle and rectangle; square and rectangle; square and angle; rectangle and circle; rectangle and semicircle; rectangle and angle. Five of these 6 differences concern the rectangle which appears to have a significantly higher sensitivity value than any of the other figures since, for the group as a whole, it gives a significant difference when compared with every other figure used. While according to the criterion of just perceived form the angle emerged significantly worse than four of the other figures (all but one!), according to the criterion of certain or correctly perceived form the angle is significantly lower than only two other figures. Hence the angle does not appear so 'bad' when the *O*s are required to report its form when certain of it. This shows, as we shall have occasion to point out later, that although the angle was not seen as readily as the other figures when just noticeable forms were reported, when it was seen it tended to be reported correctly, having fewer false reports than other figures which might have appeared 'better' from the point of view of 'apparent shape.' (Cf. Table XIV which gives the percentage of times each figure was correctly reported.)

It is difficult to summarize what we have found with respect to 'goodness' of form from the results of Form 1 and Form 2. While the angle appeared to have a lower sensitivity value than the other figures for Form 1 in the results of the group as a whole, as a matter of fact an examination of the individual results showed that the semicircle was perhaps worse, having 11 significant differences in all. The rectangle also showed 11 significant differences in the individual results but when the results were pooled in the group results, both the superiority of the rectangle and the badness of the semicircle fail to appear. On the other hand, in the group results for Form 2 the superiority of the rectangle clearly is demonstrated since it is found to have a significantly higher sensitivity value than any of the other figures. Here, too, the angle does not appear to be so bad as it was in Form 1 instructions. If we take the individual results for form 2, we find 12 significant differences in all for the angle, 11 for the rectangle, and 10 for the semicircle; the latter, like the angle, having a lower sensitivity value, in general, than the other figures. The individual results (Table VII, under Form 2) make the semicircle appear nearly as bad as the angle, but in the group results its sensitivity value is significantly lower than one figure only!

Logically, and according to statistical canons, we have no right to draw conclusions from the group results to the exclusion of the individual differences, and *vice versa*. But we may take what both methods of computing significant differences agree in, viz., the angle is not a good figure under Form 1 conditions, but is fairly good under Form 2 conditions (being little worse than the semicircle in the individual results for Form 2): if we were to choose the best figure, it would be the *rectangle* whose limen, according to both group and individual results, is significantly lower than that of any other figure.

To those who are familiar with the configurational literature it may appear strange that the circle which has been touted as such a good form fails to qualify as either a good or the best shape since it neither tends to be seen first among all the forms nor to be reported correctly at a lower liminal value than the other figures—in fact its sensitivity average is third for Form 1 and fourth for Form 2. While the circle may be a 'good' figure on the basis of physical considerations, and under other conditions of experimentation, in our experiments it never fulfilled the high expectations which the hypotheses based upon *physische Gestalten* would lead one to entertain.[23] This merely demonstrates that what is a good configuration from the point of view of physical considerations may be a mediocre or poor shape from the point of view of psychological observation.

It appears, therefore, that while there may be significant differences among forms for some *Os when they are seen*, different conclusions respecting such differences may be arrived at, depending upon whether we base them upon individual results, group results, or Form 1, or Form 2 conditions. But the possibilities governing our judgments of form have not yet been exhausted for there are still other criteria, springing directly from observation, upon which forms may be called good, bad, or indifferent.

(*4*) *Goodness of form as a matter of the criterion used.* If we scrutinize our results still further we find that a form may be good or bad on several counts, each leading to a different form as the best or worst.

[23]Thus Köhler states that the law of *Prägnanz* is exemplified in the tendency of *der Kreis als Endergebnis in der Physik wie im Gesichtsfeld besonders ausgezeichnet [zu sein]* (*Brit. J. Psychol.*, The problem of form in perception, 14, 1923, 268). It should be noted that Köhler says explicitly that the circle is preferred not only from physical considerations but in the visual field as well, as this quotation proves. In none of our experiments and by none of the numerous criteria which we are using does the circle appear *besonders ausgezeichnet*. Nor does the failure of the circle to qualify as a good or the best figure arise from the fact that we have chosen criteria unfavorable to it as a figure—we have tested each figure by every criterion that our quantitative results would allow leaving it to the facts to speak for themselves.

If we take as a criterion of goodness that figure which was reported as seen first the largest number of times in the first 50 observations on Form 1, we find from Table VIII, wherein the figures actually exposed are given horizontally and the figures reported, whether correct or not, are given vertically, that the triangle was seen 298 times; rectangle, 215; square, 215; circle, 198; semicircle, 142; and angle, 132. By this token the triangle could be taken as the "best"

TABLE VIII

REPORTS FROM ALL *O*S ON FORM

(First 50 Observations)

Figure Seen as	Figures exposed: Tri.	Rect.	Sq.	Circ.	Semi-c.	Angle	Total	Av.	σ
Tri.	189	0	0	0	52	57	298	74.5	9.23
Rect.	1	198	0	0	16	0	215	53.7	0.82
Square	0	0	180	30	1	4	215	53.7	5.80
Circle	0	0	20	170	1	7	198	49.5	5.59
Semi-c.	4	2	0	0	124	12	142	35.5	7.08
Angle	6	0	0	0	6	120	132	33.0	8.51

figure since it tends to be seen more frequently than any other form when any form at all is seen. Table VIII also shows the number of times each figure was "confused" or mistaken for every other in the first 50 observations on Form 1. Thus, reading from the top of the table to the left, we see that the triangle was reported as triangle 189 times out of 200 (4×50), but 52 times the semicircle was reported as triangle and 57 times the angle was reported as triangle, while the square and circle were not reported as triangle by any *O* at any time in the observations for Form 1. The average number of times each figure was reported as seen and the σ of this average are given in Table VIII and utilized in Table IX where the differences, the σ diff. and the D/σ diff. are shown in order to determine if there is a significant difference in the number of times each figure tended to be reported. Significant ratios, *i.e.* 3 or above, appear for every figure, but the triangle alone is significantly different from *all* the other

TABLE IX

SIGNIFICANT DIFFERENCES IN REPORTS ON FORM FOR ALL *Os*
(First 50 Observations)

		Triangle	Rectangle	Square	Circle	Semicircle
	D	20.75				
Rect.	σ diff.	4.63				
	D/σ diff.	4.48				
	D	20.75	0.00			
Square	σ diff.	5.45	2.93			
	D/σ diff.	3.81	0.00			
	D	25.00	4.25	4.25		
Circle	σ diff.	5.40	2.83	4.03		
	D/σ diff.	4.63	1.50	1.05		
	D	39.00	18.25	18.25	14.00	
Semi-c.	σ diff.	5.82	3.56	4.58	4.51	
	D/σ diff.	6.70	5.13	3.98	3.10	
	D	41.50	20.75	20.75	16.50	2.50
Angle	σ diff.	6.28	4.28	5.15	5.09	5.54
	D/σ diff.	6.61	4.85	4.03	3.24	0.45

shapes while the semicircle and angle are both significantly different (in tending to be reported less frequently than other figures) from 4 other figures. Whether the 3 significant differences each for rectangle, square, and circle mean any more than the fact that the other three figures are reported a greater (triangle) or fewer (semicircle and angle) number of times, we do not attempt to say. It should be noticed that the significant differences for the last three figures account for the significant ratios found for the first three. Certainly on the basis of these findings, different forms tend to be seen more or less frequently and hence form, if seen at all, plays a part. But since each figure was presented the same number of times every other figure was, it appears as if the conditions favoring apparent form reside as much within the organism as outside in the objective configuration. The ambiguous figures are certainly the semicircle and angle since they are apt to be called something else more frequently than the other figures. If we were to pick the best figure from the point of view of the figure which 'appears' most frequently, regardless of the form actually presented, then the triangle emerges as the best figure! Yet the theorizing of the configurationists which has been based upon the properties of physical configurations would never have led us to believe that the triangle would be preferred in perception!

Lest we generalize too hastily concerning which is the 'best' figure let us turn to the other criteria which may be used to determine the 'best' shape. If we take as the best figure that one which was reported *correctly*, or required the fewest number of repetitions for 50 *correct* reports on form (Form 2 conditions),[24] we find that the rectangle gets first place. In Table X are given the number of times

TABLE X

NUMBER OF TIMES EVERY FIGURE HAD TO BE PRESENTED TO OBTAIN 50 CORRECT REPORTS ON FORM

	Rect.	Circle	Square	Angle	Tri.	Semi-c.
D	50	56	50	70	50	74
H	51	56	62	89	58	67
M	50	65	50	53	50	68
Z	51	77	58	82	53	80
Av.	50.50	63.50	55.00	73.50	52.75	72.25
σ	1.00	8.61	5.19	10.78	3.27	5.45

each figure had to be presented to each *O* in order to get 50 correct reports on form, the averages for all *O*s and the σ of these averages. Again the angle and semicircle get a bad rating since they required the largest number of exposures before being named correctly. In Table XI are given the differences between the averages of the different figures, the σ diff. and D/σ diff. From this table we learn that the rectangle required a fewer number of exposures than three other figures, while the semicircle and angle required a larger number of exposures than three other figures, significant differences only being taken account of here. The square and circle would appear to be neither significantly good nor bad.

There are still other criteria of goodness. We have found that there are no significant differences in the amount of light necessary to see the various shapes at the lower threshold of vision, *i.e.* in the limens for light. In Table XII are given the limens in order of

[24]Here the *O*s were required to increase the intensity of the stimulus until they were sure of the form seen. If they reported they were certain and named the form incorrectly, the stimulus was repeated at a later time. Since we were trying to determine the lower threshold for 'certain' form it was necessary to adopt a procedure that would give the lowest value possible for all figures. On the other hand the number of times figures were incorrectly reported could be counted and the different figures compared not only with respect to their thresholds but also as regards the number of exposures necessary for 50 correct reports on form. Under the conditions of Form 1, on the contrary, we were interested mainly in 'apparent' form, whether correctly reported or not, so far as stimulus was concerned.

TABLE XI

SIGNIFICANT DIFFERENCES BETWEEN FIGURES IN THE NUMBER OF TIMES THEY WERE PRESENTED TO OBTAIN 50 CORRECT FORM REPORTS

		Semi-c.	Triangle	Angle	Square	Circle
	D	19.50				
Triangle	σ diff.	3.17				
	D/σ diff.	6.15				
	D	1.25	20.75			
Angle	σ diff.	6.03	5.63			
	D/σ diff.	.21	3.86			
	D	17.25	2.25	18.50		
Square	σ diff.	3.76	3.06	5.90		
	D/σ diff.	4.58	.73	3.13		
	D	8.75	10.75	10.00	8.50	
Circle	σ diff.	5.08	4.59	6.89	5.02	
	D/σ diff.	1.72	2.34	1.45	1.69	
	D	21.75	2.25	23.00	4.50	13.00
Rect.	σ diff.	2.76	1.70	5.41	2.64	4.32
	D/σ diff.	7.88	1.32	4.25	1.70	3.00

TABLE XII

LIMENS IN ORDER OF MAGNITUDE

	Observer							
	Z		*D*		*M*		*H*	
	Order							
Light	(1) Circle	294	Circle	40	Rect.	425	Tri.	2003
	(2) Angle	284	Angle	39	Angle	410	Angle	1972
	(3) Square	280	Rect.	38	Tri.	401	Circle	1943
	(4) Tri.	271	Semi-c.	34	Square	385	Rect.	1916
	(5) Rect.	264	Tri.	33	Circle	377	Square	1852
	(6) Semi.-c.	219	Square	32	Semi-c.	351	Semi-c.	1658
Form 1	(1) Rect.	33	Rect.	7.5	Tri.	15	Rect.	207
	(2) Tri.	30	Tri.	6.5	Rect.	14	Circle	147
	(3) Circle	30	Square	6.2	Square	11.5	Square	142
	(4) Square	27	Circle	5.5	Circle	11.2	Tri.	135
	(5) Angle	26	Semi-c.	4.4	Angle	9	Semi-c.	90
	(6) Semi-c.	25	Angle	4.2	Semi-c.	7	Angle	74
Form 2	(1) Rect.	7	Square	2.2	Tri.	3.7	Rect.	197
	(2) Semi-c.	6.2	Rect.	2.1	Square	3.6	Square	95
	(3) Square	6.2	Tri.	2.1	Rect.	3.4	Tri.	80
	(4) Tri.	6	Circle	1.6	Circle	3.1	Circle	77
	(5) Circle	5.9	Semi-c.	1.4	Angle	2.9	Semi-c.	60
	(6) Angle	5.5	Angle	1.1	Semi-c.	2.7	Angle	46

magnitude for the various figures and for each *O* separately; the limens being for light, Form 1, and Form 2. We have summarized the data given in Table XII for all *O*s in Table XIII, where the figures are arranged according to their average limens and average rank order for light, Form 1 and Form 2. While an order is given in Tables XII and XIII for light limens, it means little or nothing since there are no significant differences in the limens for light. But if form is perceived, either under the conditions of Form 1 or Form 2, then significant differences do appear among the various shapes. From the

TABLE XIII

ORDER OF FIGURES ACCORDING TO AVERAGE LIMEN AND AVERAGE RANK ORDER

Average Limen			Average Rank Order		
Light	Form 1	Form 2	Light	Form 1	Form 2
Tri.	Rect.	Rect.	Angle	Rect.	Rect.
Angle	Circle	Square	Circle	Tri.	Square
Circle	Tri.	Tri.	Tri.	Circle	Tri.
Rect.	Square	Circle	Rect.	Square	Circle
Square	Semi-c.	Angle	Square	Semi-c.	Semi-c.
Semi-c.	Angle	Semi-c.	Semi-c.	Angle	Angle

positions of the rectangle (first) and the semicircle and angle (occupying the last two places) in Table XIII we might conclude that the former requires the least amount of light in order to be perceived while the latter require the greatest amount of light. These conclusions hold whether the average limens are taken or the average rank order of the different figures for Form 1 and Form 2 (Table XIII). Again the rectangle emerges as the best figure, a result we should never have expected from the configurationists' hypotheses regarding the physical bases of goodness of figures, while the angle and semicircle emerge as 'bad' figures.

There is a final criterion by which we may seek to establish the goodness of shapes: the number or percentage of times each figure was reported *correctly* when it was first seen (under conditions of Form 1). We have seen how, in the first 50 reports on form (Form 1 conditions), some figures tended to be reported more often than others, hence were confused or reported incorrectly more frequently. By dividing the number of times each figure was reported as itself (*i.e.* correctly) by the total number of times it was seen, we get a measure of the correctness of report under the conditions of just barely per-

ceived forms. The requisite data, taken from Table VIII, appear in Table XIV wherein it will be found that the rectangle was reported correctly more often than any other figure and the triangle, which was reported most frequently, was confused the largest number of times with other figures. Thus the triangle which seems to be the best figure from the point of view of tendency to be present phenomenally, would be adjudged the poorest figure in so far as its tendency to be confused or reported incorrectly is concerned. It is noticeable that the angle and semicircle get second and third positions by this criterion, showing that they are not so often mistaken, when reported, as other figures. The circle, supposedly a perfect figure from physical considerations, appears third from the bottom.

TABLE XIV

PERCENTAGE OF TIMES EVERY FIGURE WAS REPORTED CORRECTLY

Rectangle	92.0	Circle	85.8
Angle	90.9	Square	83.7
Semicircle	87.3	Triangle	63.4

What then does one mean by a good figure or a poor one? What reason have we for choosing one criterion of goodness rather than another? And if the results from different criteria do not agree, what generalizations can be drawn regarding goodness of form? If observation furnishes no single, univocal criterion of goodness, recourse to physical considerations followed by inference to phenomenal properties confounds confusion the more. The circle which should be a good figure from considerations of physical *Gestalten* stands first in none of our lists according to none of our criteria. Is goodness of form a matter of simplicity, compactness, differentiation, homogeneity or what? How should these terms be interpreted, in the language of physical distributions of energy or from the standpoint of observational behavior? If we are to be guided by physical considerations, then the circle which represents a balanced, minimum distribution of forces should be the best figure as has been asserted. In observation it might be said to be simple since it is a more homogeneous figure than the rectangle and triangle. But simplicity and other descriptive concepts, as Koffka has pointed out, require functional (and quantitative) tests. By none of our functional and quantitative tests did the circle qualify as the best figure. Unanimity as to what constitutes a 'good' figure in crude observation has not yet been reached and when

we come to experimental tests of the concept its ambiguity is more apparent than ever.[25] What may be a good figure according to the judgment one makes in looking at a drawing under everyday conditions, or a good figure according to the shapes assumed by soap bubbles, or the behavior of figures in gamma movements, cannot be generalized into an all-pervading law and made the basis of a system of psychology. Worse still, the whole problem is further complicated by the fact of individual differences which we now discuss.

(*5*) *Individual differences.* The 4 *O*s in our experiments seem to fall into three groups so far as the influence of form in perception is concerned. An inspection of the individual results in Table VII for Form 1 and Form 2 shows that some *O*s are affected much more than others by the factor of form and at least one *O* is not affected at all or does not respond to form as such. Thus, for Form 1, out of a possible 60 significant differences (15 for each *O*) we find 20 each for *D* and *H*,[26] 12 for *M*, and 0 for *Z*. The results for Form 2 follow closely those for Form 1: 22 for *D*, 20 for *H*, 10 for *M*, and 4 for *Z*. It would appear that *O*s *D* and *H* are most amenable to the form factor in perception, *M* less amenable, and *Z* probably absolutely uninfluenced by form, even when forms are seen. These results, moreover, are what might be expected, both on *a priori* grounds and the findings of previous investigators: some *O*s respond rather easily to the form of a stimulus complex and all that it entails, other *O*s do not respond at all, and still others (if the 'trait' is normally distributed) fall in between the first two groups.[27] How, then, is it possible to assert that form is a primary datum for all *O*s, that it is first in the history of the individual and the race, that it is constitutive in perception and behavior? Even under the conditions most favorable to form as such, *i.e.* when the *O*s were required to report the form seen, some *O*s fail to discriminate between forms or to be affected by the shape of the stimuli. Having found that it is impossible to set up criteria governing the effectiveness of form *qua* form, since various criteria give

[25]Rupprecht Matthaei (*Das Gestaltproblem*, 1929) has discussed the difficulties with trying to settle what a 'good' configuration is. It might well be asserted that while the circle is simple it is not membered, hence not a 'good' configuration.

[26]See note 22 for the explanation of the number of significant differences when individual figures are counted and added.

[27]Differences in individual reactions to the form factor in perception have led some writers (Benussi, Seifert) to assert that we are dealing with a difference in 'type'. If so, it would be interesting to discover if there are any correlations between susceptibility to forms or lack of it and other, perhaps more complex, functions. For a further discussion of types, see H. Helson, *op. cit.*, 210 ff.

contradictory results, we now find that it is impossible to legislate for all *O*s regarding any influence of form whatsoever since some *O*s are apparently unaffected by differences in forms, even when set to observe them. We are therefore forced by our experiments to deny that the factor of form is always or even, as Köhler has tried to ameliorate the generalization, "the most important property which a whole may have." It may be a determining factor for some individuals in some types of experiment, but this is different from the assertion that it is the primary or most important condition in perceptual behavior. The broad generalizations respecting the rôle of form in perception made by the configurationists constitute another example of the fallacy of generalizing from "some to all."

Summary and Conclusions

We have found that the problem of form is an old one in psychology, having been explicitly recognized by Mach, Von Ehrenfels, Husserl, and Schumann. To the latter belongs the credit for having *consciously* said that certain characteristic properties of figures belong to them by virtue of their figural properties. But Schumann was careful to point out that the concept of form, of a unity, and such terms was ambiguous and might hide important psychological facts. One of these facts as disclosed in the present study and confirmed by the work of others, is that form is not prior or primary in visual experience since the limens for light are in every case lower than the limens for just barely perceived form and are not influenced in any detectable manner by form. So far as the light sense goes, form is not a constitutive factor and plays no part in determining the value of the lower threshold. Furthermore, the fact that it requires 25 times as much light to recognize forms correctly as to barely perceive light, and nearly twice as much light to perceive forms in a hazy, indistinct manner as compared with just noticeable light shows that the light sense is mediated by a simpler, more primitive mechanism than that for forms. The assertion that forms are first and primary in perception therefore falls to the ground.

Can generalizations be made regarding goodness of form, supposing forms are perceived? Our quantitative results show that different criteria favor different forms: the form requiring the least amount of light is the rectangle; the form reported the larger number of times on the barely perceived level is the triangle; the [illegible] confused the least number of times with other forms is the rectangle; the form

requiring the fewest number of reëxposures for 50 correct and certain reports on form is the rectangle; the form appearing neither good nor bad according to any criteria, the circle. If we were to choose the best form on the basis of all our criteria taken together, the rectangle would get first place. The circle, from the configurational point of view the best figure, never received first place according to any test, nor should we be led to expect from their considerations that the rectangle would have shown up so well.

Generalizations concerning goodness of form are still further complicated by the fact of individual differences: what emerges as the best figure according to group results does not necessarily hold for each member of the group: thus for some *O*s square and triangle have a lower limen for form than the rectangle. Finally, it appears that not all *O*s are equally affected by the form factor: one *O* showed no significant differences in her limens even when forms were being reported upon; another *O* shows so few that it is doubtful if they are significant; the other two *O*s seem to be affected by some forms and not by others.

We are thus led to conclude that while form may play some part in certain fields of perception, it has played, all in all, relatively little in these experiments designed to show its influence. That the configurationists have over-emphasized and over-generalized the importance of form as such seems to us to be borne out by the results of this study.

A GENETIC STUDY OF COLOR NAMING AND WORD READING

By Ernest Mayfield Ligon, Union College

The normal adult requires a longer time to name colors than to read the names of the colors presented as words. This fact has been known for more than 50 years, during which time a number of studies have been made of the problem and a number of theories advanced in explanation, but the problem is still unsolved. It was, therefore, in the hope of gaining further knowledge of the phenomena which would lead to the ultimate explanation, that the present study was undertaken.[1]

Experimental Procedure

The observations for the present study were made in the public schools of New London, Connecticut. The experimenters were students of psychology in Connecticut College. Each experimenter was provided with the Woodworth-Wells color naming test.[2] A word reading test was prepared, which was a typewritten sheet containing 100 words, the names of the colors on the color naming test. The two tests were made as nearly identical as possible, except that one contained the colored squares and the other the names of the colors. The two tests were arranged in reverse order so that the sequence of responses in them would not be identical. Time was taken with a stop-watch.

The experimenters were divided into two groups. Group I presented to their *S*s the color-naming test and then the word-reading test. Group II reversed this order. This was done to equalize in the averages any effects of practice and fatigue. A record was kept for every *S* of the name, sex, age, school grade, time for each test, errors, and any unusual observations. Later, class marks were obtained for two grades and intelligence test scores for seven.

The *S*s used from the 1st to the 6th grade constituted the entire student body of one public school, thus including all grades of intelligence and an approximately equal number of boys and girls. The 7th, 8th, and 9th grades were in different schools. Caution was taken to assure the presence of a random sample of each grade. In all except the 9th grade, pupils had been divided into at least two and sometimes three sections, called A, B, and C. This division had been made in an effort to

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[1]I am indebted in this study to my students at Connecticut College who acted as experimenters; to the public school authorities of New London, Conn., whose permission to work in the schools made the study possible; and to the teachers and pupils in the schools whose hearty coöperation was necessary to the success of the work.

[2]R. S. Woodworth and F. L. Wells, Association tests, *Psychol. Monog.*, 13 1911, (No. 57), 1-85.

have the best students in section A, and the poorest in C. This sectioning was also recorded in the experiment, thus affording a rough intelligence classification, which for group comparisons might be reliable.

The experiment was conducted during school hours with the most hearty coöperation of both teachers and pupils.

Results

An analysis of the data obtained will now be presented. The problems on which data are available are as follows: (*1*) a comparison of the time required for the color naming and word reading functions, with *Ss* classified both by school grade and chronological age; (*2*)

TABLE I

Average Time for Color Naming and Word Reading Tests by School Grade

(The numbers in parentheses are the medians)

Grade	No.	Colors	P.E.	Words	P.E.	Diff.	P.E.	Diff./P.E
I	76	130.2	3.1	100.6	2.9	29.6	3.4	8.7
		(123.0)		(93.0)		(30.0)		
II	83	107.4	1.7	79.6	1.7	27.8	2.0	13.9
		(107.0)		(75.0)		(32.0)		
III	69	90.7		63.2	1.5	27.5	2.1	13.1
		(89.0)	1.6	(59.0)		(30.0)		
IV	74	88.3		59.6	1.0	28.7	1.3	22.1
		(87.0)	1.2	(58.0)		(29.0)		
V	75	81.0		52.1	.6	28.9	1.0	28.9
		(80.0)	1.2	(50.0)		(30.0)		
VI	84	77.3		50.5	.7	26.8	1.3	20.6
		(75.0)	1.3	(49.5)		(25.5)		
VII	59	73.5	1.1	45.0	.5	28.5	1.1	25.9
		(72.0)		(43.5)		(28.5)		
VIII	64	68.7	1.6	45.3	.7	34.4	1.6	14.6
		(68.0)		(45.0)		(23.0)		
IX	54	69.1	1.1	45.8	.6	23.3	1.1	21.2
		(69.5)		(46.5)		(23.0)		
Total	638							

the correlation between these functions; (*3*) the relationship between each function and the difference between them; (*4*) the relationship between each function and intelligence; (*5*) the frequency of errors and the relationship between the two functions for erring; and (*6*) sex differences in the two functions.

(*1*) *Time required by the two functions.* In Tables I and II are the general results of the investigation. From them we may compare the temporal relations of the two functions.

Table I shows a comparison of all *Ss*, classified by school grades for the two functions. Attention is called to the very large number of cases, since this increases the reliability of the averages computed from them.

The average will be used as a measure of central tendency throughout the paper. The median is shown in this first table to show the very small difference between the average and the median. This justifies the exclusive use of the average as a measure of central tendency.

In Fig. 1 the data are presented graphically.

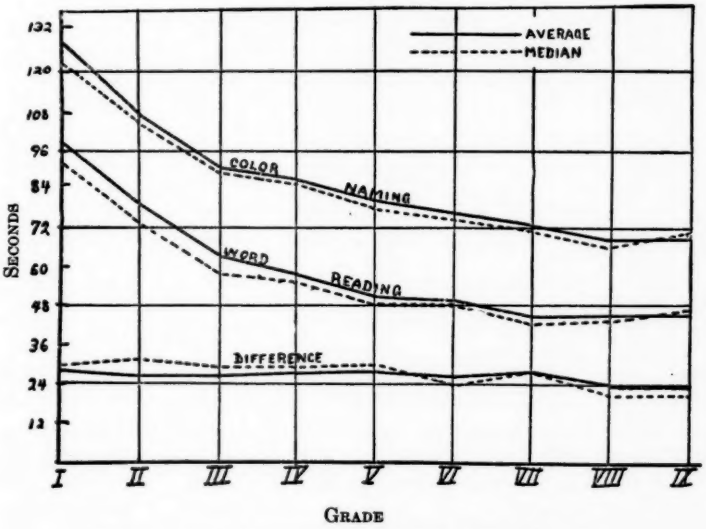

FIG. 1. COLOR NAMING COMPARED WITH WORD READING

It will be observed that in both functions there is a progressive increase in speed throughout the range tested. The improvement in the first few grades is much greater proportionately than in the last grades. This resembles most learning curves and immediately suggests that both functions contain some factor which is practiced in the normal education of the subjects.

The most striking feature of these data, however, is the approximate constancy of the difference between the two functions. The curves of the two functions are almost parallel. The curve of the difference is nearly a straight line, showing almost no decrease from the 1st to 9th grades. This difference between the two functions is the phenomenon which we are studying, and this feature of it will be of the utmost importance in its interpretation.

The probable error of the difference is so small as compared to the difference itself, that there can be no question of its reliability. It will be seen that it is always much more than four times its P.E.

TABLE II

AVERAGE TIME FOR COLOR NAMING AND WORD READING TESTS BY CHRONOLOGICAL AGES

(Ages 17 and 18 yr. not added because of the small number of cases)

Age	No.	Colors	P.E.	Words	P.E.	Diff.	P.E. Diff.	Diff./P.E.
6	16	128.8	6.0	103.5	4.7	25.3	6.9	3.7
7	54	128.9	3.8	92.6	3.0	36.3	4.8	7.5
8	78	107.2	1.8	80.6	1.6	26.6	2.4	11.0
9	52	97.0	2.3	73.4	2.7	23.6	3.5	6.7
10	74	89.6	1.5	55.0	1.1	34.6	1.9	18.2
11	78	83.5	1.1	55.4	.8	28.1	1.4	20.1
12	86	79.5	1.1	52.8	1.1	26.7	1.6	16.6
13	72	75.3	1.5	57.0	.9	18.3	1.7	10.8
14	80	74.7	1.2	48.9	1.0	25.8	1.6	16.1
15	37	70.6	1.0	46.5	.8	24.1	1.3	18.6
16	15	71.1	2.2	45.9	1.3	25.2	2.6	9.7

Table II shows the same data, with the *Ss* classified by chronological age. The range of ages included is from 6 to 16 years. There were 3 *Ss* 17 yr. of age and 1 who was 18. These were omitted in this table, both because of the unreliability of so small a group and because of the obvious selected nature of these samples, due to the fact that these *Ss* are obviously much retarded in their school progress and are therefore not representative of their age. The opposite may be true of the 6-yr. old *Ss* also, but since there is a larger group of them, they have been included.

There is very little difference between the data when classified in the two ways. In Table II the differences are slightly more variable. But it will be observed that there is no consistent tendency for the difference to become smaller as the subject grows older. Some

of the inconsistency can be explained by the presence of a larger number of boys or girls in the age group. There are sex differences, especially in the case of color naming.

The general results, then, show that older and more advanced *Ss* perform both functions faster than the younger ones, but that the superiority of the speed of word reading over color naming is approximately constant despite age or school advancement.

(*2*) *Correlation between functions.* It has been suggested that the color naming function is the same process as the word reading, plus another process. *Ss*, introspecting, often insist that this is true. If it is true, however, word reading would never be slower than color naming. There were a number of *Ss* in this experiment who did read the colors faster than the words. Also, if the hypothesis be valid, there should be a very high correlation between relative ability on the two tests. Even if the additional process does not correlate highly with the common factor, the correlation should in groups be high and fairly constant.

TABLE III

RELATIONSHIP BETWEEN THE TWO FUNCTIONS

Grade	r	P.E.	Grade	r	P.E.
I	.709	.047	VI	.506	.060
II	.588	.052	VII	.275	.087
III	.162	.077	VIII	.394	.068
IV	.594	.502	IX	.423	.080
V	.405	.068	Av.	.450	.066

Table III shows the coefficients obtained between the two functions in the several grades. While, in most cases, the relationship indicated by these coefficients is high enough to be reliable, it seems neither high enough, nor constant enough, to substantiate the foregoing hypothesis.[3]

[3]During the experiment, 17 *Ss* succeeded in naming the colors faster than they read the words. Of these, 5 were in grade I, 5 in grade II, 3 in III, and 1 each in grades IV, VI, VIII, and IX. Such scores might be due either to unusually fast color naming or unusually slow word reading. An analysis of the scores seems to indicate that both conditions were present. Of the 17 *Ss*, 12 made color naming scores that excelled the average of their respective grades, and 5 made scores poorer than these averages. For the entire 17, the average deviation from the mean of their grades was −12.1 sec. On the other hand, all but one *S* read words slower than the average of their groups. The average deviation from the mean of their grades was +44.6. Of the 17 *Ss*, 5 excelled their groups in color naming by larger amounts than they proved inferior in word reading; the other 12 reversed this condition. These results indicate that both factors then combined to produce these unusual scores, but that inferiority in word reading was the more important of the two.

The correlation between the functions is always positive and usually fairly high, showing that the two functions are certainly not entirely independent. Yet, the variability of the coefficient and the low level to which it drops, also, indicates that there are some factors at least in which the two functions vary independently.

(3) *Relationship of functions to differences.* Since the difference between the functions forms the central problem of this study, correlations were computed between it and each of the functions. The difference was computed by subtracting time for word reading from time for color naming. Those cases in which the word reading was the larger number were treated as negative differences. Table IV shows these results.

TABLE IV

CORRELATIONS BETWEEN FUNCTIONS AND DIFFERENCES

CN refers to color naming; WR to word reading; Diff. to the difference between the two.

Grade	r CN-Diff.	P.E.	r WR-Diff.	P.E.	σ CN/σ WR
I	.500	.055	−.220	.083	1.08
II	.460	.055	−.400	.063	1.00
III	.663	.047	−.599	.051	1.09
IV	.644	.047	−.284	.073	1.14
V	.944	.008	−.122	.079	1.90
VI	.763	.032	−.062	.079	1.73
VII	.855	.025	−.249	.083	1.94
VIII	.746	.038	−.323	.077	1.38
IX	.893	.018	−.222	.091	1.64
Av.	.717		−.274		

Rank Square Difference Correlations

First series	Second series	Rho
r CN-Diff.	r WR-Diff.	−.49
r CN-WR	r CN-Diff.	−.56
r CN-WR	r WR-Diff.	−.33
ratio of σ	r CN-Diff.	+.90
ratio of σ	r WR-Diff.	−.55
σ CN	r CN-WR	+.44
σ WR	r CN-WR	+.47

*Professor F. C. Chillrud checked these coefficients by computing them by formula instead of actual plotting as I have done.

Let us examine this table carefully, for in it there are many clues leading to the solution of our central problem; namely, the explanation of the difference between the two functions.

If the two functions were entirely independent, color naming being simply a longer process than word reading, certain results could be expected. In the first place, color naming would correlate positively with the difference, since (except for the chance variation in word reading time) the greater the time required for color naming, the greater would be the difference. In the same way, in the second place, word reading should correlate negatively with the difference, since (except for chance variation in color naming time) the longer the time required for word reading, the smaller would be the difference. In each case, the smaller the relative variability of the other, the higher the correlation. All of these conditions are fulfilled in the above data. Color naming correlates positively with the difference. The higher the ratio between the standard deviations of the two functions, the higher the correlation. The rank square difference coefficient between this ratio and the size of the correlation is 0.90. Word reading correlates negatively with the difference, and the corresponding rank square difference score is $=0.55$.[4]

There is, however, one further condition which should be fulfilled if the two functions are entirely independent; namely, there ought to be no relationship between them. There is, however, a fairly high relationship between them—it averages 0.450 (see Table III). This would indicate a definite relationship between them. The two functions are not independent. If the assumption is made that they are very highly related, so that excellence in one signifies excellence in the other and vice versa, there ought to be much higher coefficients between the two functions than are found, and word reading ought to correlate positively with the difference; for in this case, the worse the score for word reading, the worse ought color naming time to be proportionately and the wider the difference between them. This condition is not found. This hypothesis cannot be maintained.

It is obvious then that the two functions cannot be regarded as simple entities, since they are both related and unrelated to each other. As will be pointed out in the conclusion, this practically eliminates the 'practice hypothesis.' Let us consider other possibilities.

[4]It will be observed that the ratio between the σ is computed by dividing the σ for color naming by that of word reading. Hence the greater the ratio, the relatively greater is the σ of color naming to that of word reading.

Suppose the color naming function is made up of two factors, in one of which it is entirely independent of the word reading function and in one of which it is identical. The latter may be referred to as the common factor and the former as the color naming special factor. The difference would then be a part of this color naming special factor. This would account for the high correlation between the color naming function and the difference and, since there is a common factor, between color naming and word reading.

But, why the negative relationship between word reading and the difference? If the common factor and color naming special factor were entirely independent, there ought to be a zero relationship between them. The only answer is that word reading is not entirely constituted of the common factor, but also has a special factor, which though very much smaller and less variable than the color naming special factor, influences the difference, and hence correlates negatively with it.

The common factor, then, causes the two functions to correlate with each other. The special factors cause this relationship to be low. It is easy to see that the common factor constitutes most of the word reading function, since the effect of the word reading special factor has so little influence upon the difference, as indicated by the correlation between word reading and the difference.

It is in the suggesting of this word reading special factor that I have entirely departed from previous hypotheses. It not only permits an explanation of the data just discussed, but it allows for those cases in which a longer time was required for word reading than for color naming.

That the two special factors are entirely independent is indicated, not only by the low correlation between the functions, but by the fact that the more each varies, the higher its correlation with the difference. Obviously, since the common factor would vary equally in the two functions, any difference in the variability of the two must be attributed to the special factors. The ratio between the σ of the two functions, then, is a statement of the variability of the color naming special factor as compared to the word reading special factor. If they are independent, the higher this ratio, the greater the relative variability of the color naming special factor and the higher will color naming correlate with the difference and the lower will word reading do so. The rank square difference coefficient between

this ratio and the r of color naming with difference is 0.90; between this ratio and the r of word reading with difference is −0.55.

The question also arises, as to whether the common factor is very variable or not. Any difference in variability between the two functions must be attributed to the color naming special factor. But of the variability of the entire word reading function, is it all in the common factor, in the special factor, or in both?

If all the variability were in the special factors, since they vary independently of each other, the larger this variability the smaller would be the correlation between the two functions. In this case the rank square difference coefficient between the size of the standard deviations and the r between the functions would be negative. In the case of color naming it is 0.439 and for word reading 0.470. This indicates that the greater the variability of the functions the greater the relationship between them. Hence a greater part of the variability must be in the common factor, a very little in the word reading special factor, and the same amount plus the difference in variability in the color naming special factor.

Since, according to this hypothesis, the common factor determines the relationship between the functions, it follows that the higher the correlation between them, the greater the influence of the common factor and the less the influence of the special factors. This would be indicated in their relationship to the difference. Hence the higher the correlation between the two functions, the lower ought to be the correlation between the functions and the difference. The rank square difference coefficients which express this are in the case of color naming −0.37 and for word reading −0.33.

It also follows, that since there is improvement in both functions with age, but with a constant difference, that it is the common factor which improves, whereas the special factors do so very little. This would indicate that the special are organic in their nature, whereas the common factor is a learned reaction.

(*4*) *Intelligence.* It will be valuable for the understanding of the problem, to find out whether either or both functions are related to intelligence. Three measures were secured in this experiment which might be expected to be measures of intelligence. In 2 grades, class marks for the year were obtained. In 7 of the 9 grades, intelligence test scores were available. In all the grades, except the 9th, there were two or three sections of each grade, made on the basis of school success. We have very little confidence in any of the three sources

as accurate indices of intelligence. Group scores should, however, be more reliable than individual and if the same results are obtained from all three sources, that in itself is evidence of reliability.

Class marks were available for the 7th and 8th grades. The correlations obtained with them are as follows: color naming with class marks; 8th grade −0.127, P.E. .086; seventh grade, −0.110, P.E. .094; for word reading with class marks, eighth grade −0.328, P.E. .076; seventh grade 0.099, P.E. .094. It will be remembered, of course, that a negative correlation indicates a positive relationship. We should expect that if the tests are indicators of school success or vice versa that high negative coefficients would be obtained. It is obvious that this is not true. Three of the 4 coefficients are approximately zero.

TABLE V

CORRELATION BETWEEN IQ AND THE TWO FUNCTIONS

Grade	r. IQ-CN	P.E.	r. IQ-WR	P.E.
I	−.148	.093	−.420	.080
II	−.049	.087	−.324	.079
III	.125	.094	−.006	.095
IV	−.026	.095	−.285	.086
V	.076	.122	.146	.120
VII	−.014	.095	−.004	.095
VIII	.192	.084	−.143	.085
Av.	.021		−.146	

An examination of this table only adds to the conclusions drawn from the class mark correlations. There are only three coefficients with any considerable size. It may be noted that both the significant coefficient in connection with school marks and the three significant scores in this table are in the word reading function and none of them in color naming.

There is one further line of evidence in connection with this problem. The average scores for the two functions may be obtained for the sections of each grade. Most of the grades were divided into three sections; three had only two sections; and the ninth grade was not divided at all. It has therefore been omitted from Table VI, which contains these results.

There is more indication of intelligence playing a part in this group comparison than in either of the other methods of comparison. The same tendency appears, however. While there is some tendency in color naming for section A to excel section B, and B to excel C,

it is by no means a marked tendency and has several exceptions. In word reading the tendency is much more pronounced. There is no definite trend in the comparison of differences at all.

TABLE VI

AVERAGES FOR SECTIONS OF SCHOOL GRADES

	Color Naming			Word Reading			Differences		
Grade	A	B	C	A	B	C	A	B	C
I	118.7	118.2	133.7	85.3	92.7	106.8	33.3	25.5	26.9
II	103.9	105.6	116.3	72.2	85.0	86.5	31.7	20.6	29.8
III	92.2	89.8	—	64.1	62.6	—	28.1	27.2	—
IV	85.4	88.7	94.9	55.0	61.4	67.0	30.4	27.3	27.9
V	79.3	82.8	—	51.3	52.9	—	28.0	29.0	—
VI	74.1	76.0	79.4	48.6	51.5	53.4	25.5	24.5	26.0
VII	73.8	73.4	—	45.7	42.7	—	28.1	30.7	—
VIII	67.2	73.2	69.6	41.9	47.7	45.9	25.3	25.5	23.7

From these data, the conclusions seem tenable that the common factor is related to intelligence, and that neither of the special factors are appreciably so related. This is indicated by the closer relationship between measures of intelligence with word reading, in which the common factor predominates, than with color naming, where the common factor is a much smaller portion. That the color naming special factor is very little related to intelligence is indicated by the complete absence of significant differences in the case of the different scores. The word reading special factor is likely to be the cause of the relatively low correlation between intelligence and word reading.

(*5*) *Errors.* A record was kept of the number of errors made by the *S*s taking the tests. An analysis of these records shows some interesting features, which will be valuable in the explanation of the phenomenon.

Table VII shows that the percentage of *S*s making errors is larger in color naming than in word reading. It shows that there is comparatively little improvement in the percentage making errors in either function through the nine grades studied. The average number of errors made by those who made errors is very strikingly constant for all grades in color naming. It is more variable for word reading, though showing little signs of improvement. In most cases, the average error for the erring is greater in color naming than in word reading.

For the group, the average error for color naming is always much greater than for word reading, due to the high percentage of *Ss* who make errors in color naming.

Since the common factor has been shown to improve throughout the 9 grades, and since there is almost no sign of improvement in eliminating errors, we are almost forced to assume that the errors are found largely in the special factors. The rank square difference correlation between the averages of the errors for those erring in the two functions is 0.30, which is surprisingly small, indicating a very low relationship between the two functions in the matter of erring.

TABLE VII

ERRORS IN THE TWO FUNCTIONS

Grade	Color Naming			Word Reading		
		Av. of			Av. of	
	% erring	erring group	entire group	% erring	erring group	entire group
I	71	2.40	1.71	50	2.65	1.36
II	63	2.76	1.73	31	1.69	.49
III	65	2.63	1.70	29	2.18	.61
IV	59	2.39	1.40	30	1.90	.59
V	55	2.89	1.59	25	2.47	.42
VI	68	2.65	1.81	29	1.68	.49
VII	58	2.40	1.41	15	2.33	.35
VIII	57	2.17	1.25	29	1.78	.52
IX	53	2.14	1.13	31	1.47	.46

It seems especially desirable to establish the last point. The rank square difference coefficient is hardly sufficient for this purpose. Recourse was taken to the individual cases. A table has been prepared, Table VIII, in which the average number of errors made in word reading is computed for those making a given number of errors in color naming. Thus in the first column are found individuals who made no errors in color naming; in the second column the average of those who made only one error in color naming, and so on. The number of cases has been placed at the bottom, so that an estimate of the reliability of the data obtained may be made.

An examination of this table shows that there is a very slight tendency for the errors made in one function to predict what will be made in the other. Even the averages, however, show great inconsistency. Certainly this table affords strong evidence of the high

degree of independence of the two functions. Since it may be assumed that most of the errors are due to the special factors, this still further establishes the comparative independence of these. That there is a very slight correlation indicates that there may be some tendency to error in the common factor.

TABLE VIII

RELATIONSHIP BETWEEN ERRORS IN THE TWO FUNCTIONS

	Errors in color naming										
Grade / Errors in word reading	0	1	2	3	4	5	6	7	8	9	10
I	.4	.8	2.0	2.2	.0	2.0	1.0				
II	.2	.3	.4	.7	1.1	2.0	2.0	2.0	.0	2.0	
III	.3	1.2	.6	.4	1.0	.0					
IV	.5	.5	.5	.5	.0	1.0	.0	7.0			
V	.1	.7	.6	.8	.0	.3	.0			.2	
VI	.2	.4	.4	1.1	.4	.8			2.0		2.0
VII	.1	.5	.4	.0	2.0	1.5	.0				.0
VIII	.4	.4	.7	.8	1.0		2.0				
IX	.3	.5	.6	1.0	2.0		.0		2.0		
Av.	.3	.6	.7	.8	.7	1.3	.8	5.5	1.0	1.0	1.0
No.	249	137	107	69	33	23	10	2	4	2	2

TABLE IX

SEX DIFFERENCES IN FUNCTIONS BY SCHOOL GRADE

Bold face type indicates those instances in which the difference is more than four times the P.E. and so statistically reliable

	Color naming				Word reading				Differences			
Grade	Boys	Girls	Diff.	P.E.	Boys	Girls	Diff.	P.E.	Boys	Girls	Diff.	P.E.
I	139.7	121.0	**18.7**	**3.1**	113.1	91.7	**21.4**	**2.9**	26.6	29.3	−2.7	4.2
II	108.5	107.4	1.1	1.7	83.1	76.7	6.4	1.7	25.4	30.7	−5.3	2.4
III	93.4	86.9	**7.5**	**1.6**	60.3	66.8	**−6.5**	**1.5**	33.1	20.1	**13.0**	**2.2**
IV	89.8	86.8	3.0	1.2	59.9	59.3	.6	1.0	29.9	27.5	2.4	1.6
V	85.5	76.9	**9.6**	**1.2**	52.0	52.3	−.3	.6	33.5	24.6	**8.9**	**1.3**
VI	84.3	72.4	**11.9**	**1.3**	51.7	49.8	1.9	.7	32.6	22.6	**10.0**	**1.5**
VII	78.4	68.9	**9.5**	**1.1**	43.8	44.8	−1.0	.5	30.5	24.1	**6.4**	**1.2**
VIII	72.5	64.3	**8.2**	**1.6**	45.8	44.9	.9	.7	26.7	19.4	**7.3**	**1.7**
IX	71.8	67.2	**4.6**	**1.1**	48.4	46.6	1.8	.6	23.4	20.6	2.8	1.3

(*6*) *Sex differences.* Previous investigators have found girls superior to boys in these functions. No extensive set of data has, however, been available for studying these differences.

Table IX indicates that there are very much greater sex differences in color naming than in word reading. The girls excelled the boys in color naming in every grade, with statistically reliable superiority in 7 of the 9 grades. In word

reading, on the other hand, there were 3 grades in which the boys excelled, and in only one did the girls have a reliable difference in their favor.

The logical deduction from this would be that girls are superior to the boys in respect to the color naming special factor, not in the common factor, with the word reading special factor in doubt. In a comparison of the differences, which constitute the major portion of the color naming special factor, we find the girls with a decided advantage except in the first two grades. There is evidence that the greater the influence of the color naming special factor, the greater the superiority of girls; and the greater the influence of the word reading special factor, the greater the superiority of boys.

TABLE X

Sexes Compared in Making Errors

(Italics indicate sex making best score when difference is not reliable; bold face indicates sex making best score when difference is reliable)

	Color Naming						Word Reading					
	% erring		Av. erring		Av. group		% erring		Av. erring		Av. group	
Grade	B	G	B	G	B	G	B	G	B	G	B	G
I	*61*	63	*2.3*	2.6	*1.4*	1.6	47	*43*	3.3	*2.0*	1.6	*.9*
II	63	**40**	**2.7**	3.1	1.7	**1.2**	40	**26**	1.5	1.6	.6	*.4*
III	**49**	63	*2.1*	2.4	**1.0**	1.5	*23*	27	*2.0*	2.3	*.5*	.6
IV	63	**47**	*2.4*	2.5	1.5	*1.2*	38	15	**1.7**	2.6	.7	*.4*
V	62	**45**	**2.8**	3.4	1.8	*1.5*	24	*23*	1.7	1.7	.4	*.4*
VI	63	*61*	2.8	*2.4*	1.7	*1.5*	31	**24**	*1.6*	1.8	.5	.4
VII	64	**50**	2.8	**2.0**	1.8	**1.0**	*14*	16	**2.0**	2.6	*.3*	.4
VIII	59	**50**	2.5	**1.7**	1.5	.9	29	*27*	1.8	*1.8*	.5	*.5*
IX	57	**52**	2.3	*2.0*	1.3	*1.0*	*29*	32	*1.4*	1.5	*.4*	.5

Table X shows that the girls excel in both functions in the percentage of the availing errors. This is especially true in respect to color naming. In regard to the average errors made by those making errors, the boys excelled in the first 5 grades and the girls in the last 4, in color naming. The differences in word reading were variable and negligible. The average errors for the groups give the girls an advantage, especially in color naming. This shows that it is in the number of those making errors rather than in errors made in which the girls excel. We may conclude, then, that girls show a reliable superiority to boys in the color naming special factor. This advantage seems to lie, not in a general superiority of all girls over all boys, but in the greater percentage of girls who make a minimum of errors. The superiority of boys over girls in the first 5 grades in average error made by the erring and the reverse in the last 4, might seem to be an interesting phenomenon in need of interpretation. The differences, however, are not great at any time. Carelessness may be found more in girls and older boys. This may account for the fact.

It was observed that girls tended to be more advanced in school for their ages than did boys. This made it desirable to study these sex differences by ages as well as by grades. It might be possible that the superiority of girls over boys is entirely due to their age. Or on the other hand, if given equal age, the boys may not be inferior to the girls in color naming. Table XI shows the differences when computed on the basis of age, and its reliability.

TABLE XI

SEX DIFFERENCES BY CHRONOLOGICAL AGES

(Bold face type indicates a reliable difference)

Age	Av. grade			Color Naming			Word Reading			D-D
	B	G	Diff.	Diff.	P.E.	D/P.E.$_D$	Diff.	P.E.	D/P.E.$_D$	
6	1.00	1.08	.08	15.0	15.1	1.0	22.1	15.0	1.5	−7.1
7	1.32	1.44	.12	28.4	8.8	3.2	24.6	7.7	3.2	3.9
8	1.85	2.08	.23	12.6	3.5	3.6	13.6	3.4	**4.0**	−0.9
9	2.68	3.13	.45	7.6	4.7	1.6	0.1	5.3	0.0	7.6
10	3.74	3.96	.22	10.9	3.2	3.4	1.5	2.3	0.6	13.4
11	4.46	4.70	.24	11.1	2.1	**5.3**	−0.9	1.8	0.5	12.0
12	5.58	5.81	.23	11.2	2.0	**5.6**	3.9	2.3	1.7	17.3
13	6.50	6.78	.28	3.6	2.9	1.3	1.3	1.7	0.8	1.4
14	7.03	7.36	.33	10.3	2.4	**4.3**	0.3	1.9	0.2	9.9
15	8.59	8.40	−.19	13.6	2.0	**6.8**	3.8	1.8	2.1	9.9
16	8.73	8.75	.02	6.2	3.5	1.8	5.9	3.3	1.8	0.3

When this table is compared with Table IX, the general trends are found. The girls excel still more in color naming. The girls show a little more advantage in word reading than in the other classification. This would indicate that the color naming special factor is a product of age and organic, since when girls are given an equal advantage in age, their superiority is greater. On the other hand, the common factor seems to be in part at least subject to the influence of training, since girls show to better advantage when classified by age, where they have the advantage in school advancement.

A study of sex differences, then, shows the girls to be superior in the color naming special factor, regardless of classification. The common factor seems to be subject to school advancement, as is indicated when the subjects are classified both by grades and by ages.

Discussion. The first hypothesis intended to explain the phenomenon was suggested by Cattell.[5] He states the so-called 'practice hypothesis.' "The association between the color word and its name has been repeated so often that it has become automatic, whereas in the case of colors a voluntary effort is required."[6] When the data were secured from adult *Ss* only, this hypothesis was difficult to refute. The only effort was made by Brown who had his *Ss* practice for long periods of time.[7] On the basis of his results, he decided against the practice hypothesis, although as Lund points out, his data can be interpreted in either way.[8]

[5] J. McK. Cattell, The time it takes to see and name objects, *Mind*, 11, 1886, 63-65.

[6] *Ibid.*, 65.

[7] Warner Brown, Practice in associating color-names with colors, *Psychol. Rev.*, 22, 1915, 45-55.

[8] F. H. Lund, The rôle of practice in the speed of association, *J. Exper. Psychol.*, 10, 1927, 424-433.

The final test of the practice hypothesis could only be made by a genetic study. This Lund did. If the difference between color naming and word reading is entirely due to practice, children before the school age ought to read the colors faster than the words. But when reading habits are acquired, since words are used so much oftener than color names, we would expect the two curves to cross. This is exactly what Lund concludes upon the basis of his experiment.[9] Starting with 5-year-old children, he tested *Ss* up to 19 yr. of age. Unfortunately he does not show a table, but presents only a graph, showing the curves of the two functions. I have endeavored to construct a difference curve from his graph, represented by the solid line in Fig. 2.

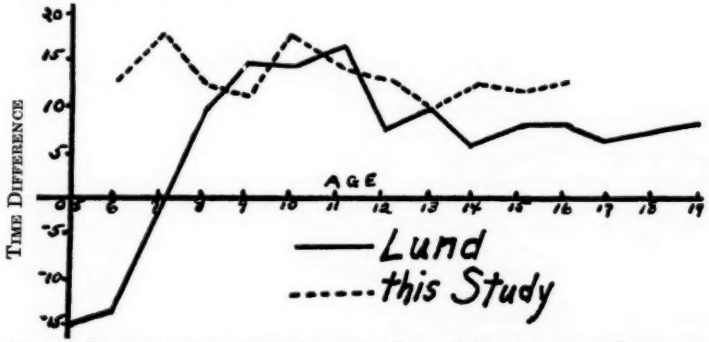

FIG. 2. COMPARISON OF THE 'DIFFERENCE CURVES' OBTAINED FROM LUND AND AND FROM THIS STUDY

(The 'Difference Curve' is obtained by subtracting the time required for word reading from that required for color naming)

He used 50 colors, whereas I used 100. I have, therefore, divided my differences in half. It is well known that the first half of the color naming test is faster than the second half, so that my corrected differences should be slightly greater than his. It is apparent that except for 6- and 7-yr. old *Ss*, we have obtained approximately the same results. At the 6-yr. old level, I used *Ss* already in the first grade. I do not know what type of *Ss* he used. But at the 7-yr. old level, where his would also be in the public school, I cannot account for our very wide disagreement. He used, however, only 6 or 7 *Ss* at each level. This would give much greater reliability to my results.

[9] *Ibid.*, 433.

It should be noted, however, that after the 8-yr. old level, his differences are almost constant, showing no gradual increase as would be expected on the basis of his hypothesis.

The 'practice hypothesis' assumes the independence of the two functions. The correlations found between them at the various grade levels averaged 0.450. These two facts seem to me to be quite complete proof that the 'practice hypothesis' is not tenable.

There is no doubt that both functions can be improved by practice. But it is the common factor which is practiced and not the special factors. The practice hypothesis cannot explain the difference between the functions.

Brown considered the possibility of the color naming function including the word reading function plus another factor. and found that this was untenable.[10] The data of my experiment also indicate that such an hypothesis cannot be supported. The fact that some *S*s read colors faster than words is one such bit of evidence.

Hollingworth suggests that there is a time required for articulation, plus, in the case of color naming, an association time.[11] This is an hypothesis, which like the preceding one, is refuted with the same facts. In discussing articulation he speaks of the mispronunciation of the words in color naming, such as "bleen," "rellow," and the like. But he uses the word reading test as an articulation test, in which no such errors occur. He seems to neglect the necessity of an association time in the word reading test, and assumes that the association time for color naming is sufficient explanation of the difference. This is refuted by the constancy of the difference. If association were the only difference, time and practice should narrow this difference. Such is not the case.

In the study made by Gates, it is significant to note that the test showing the highest relationship with color naming was tapping, a much more structural function than the other tests used, adding, word building and multiplying tests.[12] This adds evidence to my contention that the color naming special factor is structural.

Garrett and Lemmon apparently abandon the 'practice hypothesis' entirely.[13] They assert that the difference is due to an inter-

[10] *Op. cit.*, 45 ff.

[11] H. L. Hollingworth, Articulation and association, *J. Educ. Psychol.*, 6, 1915, 99-105.

[12] G. S. Gates, Individual differences as affected by practice, *Arch. Psychol.*, 8, 1922, (no. 58), 1-74.

[13] H. E. Garrett and V. W. Lemmon, An analysis of several well-known tests, *J. Appl. Psychol.*, 8, 1924, 424-438.

ference factor. This factor is based on the introspective evidence of their *S*s. They measure it by obtaining a ratio between the color naming test and a color finding test. The latter consists of asking the *S* to pick out all the squares of the same color at once. They assert that the *S*s can recognize the colors fast enough, as is shown by their speed in color finding, but are prevented by the interference factor, from naming them. The question that arises is, why does this interference factor not operate in the word reading test? This they do not consider. If *S* can recognize the color as fast as the word, why can he not pronounce it as fast? To be sure, there is an interference factor, but it is in the color naming test and has nothing to do with the articulation process in pronunciation.

Since none of these hypotheses are supported by the data of this investigation, it seems desirable that a new hypothesis be formulated, which will explain these data. Such an hypothesis can only be tentative, awaiting further experimentation for its testing. Three experiments are now under way in the Union Laboratory, which will have very direct bearing upon this problem when they are completed.

As I have already indicated, the three-factor hypothesis seems tenable in view of these data. The question that now arises is what are these factors? What is the common factor? What are each of the two special factors? Before answering, even tentatively, it must be stated that the common factor may be and probably is a group of factors, which are common to both functions. The same may be said of the special factors.

Perhaps an analysis of each function may help. In color naming, the *S* must recognize the color, must recall its name, and must articulate its name. In word reading, the *S* must recognize the word, must recall its pronunciation, and must articulate it. I recognize that this analysis is entirely logical and may not be an actual description of the psychological process at all.

In looking for the common factor, there seems little doubt that the articulation time for both is common. Whether this is sufficiently influenced by practice to explain the preceding data is doubtful. The recognition process may be so similar that it will be common except for the difference in reaction time to the colors as contrasted with the words. A reaction time experiment, which is now being done, may give evidence that will help answer this question.

The recognition process and the articulation process are both evidently necessary to either function. But what happens between the two seems likely to form the basis for the special factors. It seems logical to assume that it requires more time to translate a recognized color into the spoken color name, than to translate a recognized word into a spoken word. That this should make color naming slower than word reading is a very reasonable assumption. But why the difference between the translation time for the two functions should have an almost constant value irrespective of age and educational advancement is difficult to see.

This seems as far as it is possible to go toward an hypothesis to explain the phenomenon, until further data are secured from the experiments now in progress.

Summary and Conclusion

The Woodworth-Wells color naming and a word reading test, consisting of a sheet on which the color names as found in the color naming test were listed in the same order, was given to 638 *Ss* ranging from the 1st to 9th grades in school and from 6 to 18 years of age.

(1) There was a progressive improvement in the performance of both functions throughout the range of those tested.

(2) The differences between the two functions remained remarkably constant throughout the 9 grades. Color naming was consistently poorer than word reading. Only 17 *Ss* gave opposite results.

(3) There was a fairly high correlation between the two functions. The relationship between color naming and the difference between the functions was positive and high, and that between the difference and word reading was negative and relatively low.

(4) The assumption that there was a common factor for the two functions and special factors peculiar to each was defended statistically. If this assumption is true, there was a very low correlation between the two special factors and between each special factor and the common factor.

(5) An analysis of the errors indicated that they were in the special factors more than in the common factor.

(6) Measures of intelligence correlated low with the two functions. There was evidence of a higher correlation with the common factor than with the special factors.

(7) The low relationship between tendency to err in the two functions added evidence of the independence of the special factors.

(8) Girls excelled the boys consistently in color naming. No great differences were found in word reading.

While previously suggested hypotheses for the ability to read the words faster than the colors have been shown untenable in the light of the data of this study, no new hypothesis beyond the three-factor theory could be offered. Unquestionably recognition, mental translation of the stimulus into the response, and articulation are factors likely to be found among the three factors.

UNSYNTHETIC MODES OF THINKING AMONG ADULTS: A DISCUSSION OF PIAGET'S CONCEPTS

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The difference between the modes of 'logical' or coherent thinking in the child and in the adult, as revealed through the medium of verbal exchange, is generally conceded to be characterized by inability to analyze and discriminate, to use integrated symbols, to form noetic syntheses. According to Piaget,[1] this defective synthesis on the part of the child manifests itself not only by way of too little synthesis, by inadequate expressing of relations of cause and effect, by irrelevant sequences in the train of thought, but also by exaggerated synthesis, by the tendency to bind everything together and to justify by ingenious devices. These two 'prelogical structures,' lack of synthesis or juxtaposition, and excessive synthesis or syncretism,[2] are due not merely to lack of maturity on the part of the child, to insufficient scholastic training in the use of grammatical forms and symbols, to inexperience in the world around him, but also and primarily, as Piaget believes, to the fact that the child is not entirely free from the 'egocentrism' that characterizes all autistic thinking, dreaming and day-dreaming, as opposed to 'logical' and synthetic thinking.[3] Because of this self-centered attitude, the child employs in his verbal communications juxtaposed and syncretic modes of expression, since he is under no compulsion to understand or make himself understood. It is only through the interplay of greater maturation and the exigencies of the world around him that he is shaken out of his egocentric point of view into a more socialized and objective one.

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[1] J. Piaget, *Le jugement et le raisonnement chez l'enfant* (referred to hereafter as *JR*), 1924, 73-81.

[2] Juxtapositions and syncretisms are found not only at the level of verbal intelligence or of logical thought, but at the perceptual level, the level of sensori-motor understanding, J R, 77.

[3] Piaget places the 'logical' thinking of the child intermediate between the autistic thinking of child and adult, and adult 'logical' thinking. In fact, he shows that syncretism is a link between the Freudian mechanisms of condensation and displacement and the generalizations and abstractions of synthetic thought. *Le langage et la pensée chez l'enfant* (hereafter referred to as *LP*), 1923, 207.

Now Piaget recognizes the fact that the adult is not entirely free from 'egocentrism' when he thinks logically, so that juxtapositions and syncretisms may persist in his verbal exchanges, particularly in novel or complex situations, or under the stress and strain of emotion (bad humor or passion), which reduce him to the level of infantile or semi-autistic psychological functioning.[4] But from his observations of and experiments on children at the Institut J. J. Rousseau, Piaget was struck by the apparent frequency with which these unsynthetic verbal expressions occurred among children; predominantly at the younger age levels while disappearing at the older ones.

Unfortunately, Piaget did not make use of his technique of investigation[5] in analogous forms on adults or on other groups of children before drawing his conclusions.[6] Isaacs, at the Malting House School, working with a group of young children superior intellectually and in cultural background to the children at Geneva, employed Piaget's method of recording conversation, questions, and explanations. She found some instances of egocentric or syncretic logic, but no hard and fast "mental structures" of first, egocentrism, then socialized logic; but rather a "continuous advance in the scope and clarity of noetic synthesis." Isaacs believes as does Piaget that the adult may also be egocentric, particularly in affective situations, unfamiliar issues, when taken unawares, or influenced by religious prejudice or social tradition.[7]

In a recent article,[8] Piaget has replied to Isaacs, admitting that the age levels where childish structures of thought disappear are certainly dependent on the intellectual capacity of the child, as well as on his cultural heritage; and furthermore, he adds, it is perfectly possible for juxtapositions and syncretisms in different fields of interest, of conceptual knowledge, or at different levels of 'consciousness' (sensori-motor or verbo-intellectual), to disappear by means of a stagger system.[9]

One investigator, Huang at Smith College, had the advantage of placing both children and adults in an identical experimental setting.[10] He employed one of Piaget's methods, that of asking the *Ss* for the causal explanation of strange phenomena (conjurer's tricks, optical illusions, a less familiar physical force opposed to a better known one) that were presented to them. His *Ss* consisted of a group of young children from professional families and a small number of college students. He was interested primarily in getting at attitudes and con-

[4] *LP*, 131; *JR*, 79.

[5] Piaget employed three methods: (1) the recording and analyzing of spontaneous conversations of children; (2) the recording and analyzing of children's answers to adult questions; and (3) the giving of tests and tasks to be performed.

[6] In justice to Piaget, it should be said that he is not blind to this defect.

[7] S. Isaacs, *Intellectual Growth in Young Children*, 1930, 92-93.

[8] Le développement intellectuel chez les jeunes enfants, *Mind*, 40, 1931, 137-160.

[9] In his books, Piaget certainly gives the impression that he accepts definite age levels; *i.e.* juxtapositions and syncretisms of perception disappearing at the age of 7-8 yr., and of verbal intelligence at 11-12 yr.

[10] I. Huang, Children's explanations of strange phenomena, *Smith Coll. Studies in Psychol.*, 1930, No. 1.

cepts (magical or natural), but he also discussed the question of logical thinking. He contended that the adult was more capable of synthesis than was the child, but that in the case of both there was distortion of the material, "modifications, supplementations and falsifications of the data of observation and memory because of the subjective character of the explanations."[11] Huang did not discuss how far these distortions were in the nature of syncretisms, but he did say that he found both the children and the college students capable of explaining cause and effect as far as their knowledge and experience allowed, and of not giving mere juxtaposition of ideas not implying active causation.

The preceding discussion suggests that the presence or absence of syncretic and juxtaposed verbal expressions is not entirely a matter of whether child or adult is doing the understanding and thinking, but dependent rather on the type of individual (child or adult), the extent of development of his psychological functions (*i.e.* intelligence), his past experiences, his organic set-up at the time of being observed, and his cultural heritage. Therefore any results obtained will vary with the nature, the difficulty, and the complexity of the material handled by the individual.

For this reason, we decided to repeat some of Piaget's work on a group of adults, not as Huang had done by presenting identical material to both children and adults, but by offering an experimental setting which would be considered difficult for the *S*s in question. This method would exclude any direct comparison with children, but would reveal whether and to what degree an adult resorts to the use of juxtapositions and syncretisms in his language communications, in a particular setting where presumably he is not day-dreaming or thrown off his guard by any appreciable emotional attitude.

Method and Procedure

One of Piaget's procedures, which he believed to be successful, in getting at the understanding and thinking of children, particularly in bringing out differences in the use of syncretisms and juxtapositions at two different age levels (6–7 and 7–8 yr.), was that of having stories repeated from one *S* to another.[12] *E* told a child he would read him a story to see how well he could tell it to another child. The first child, the 'explainer,' was sometimes asked to repeat his story to *E* before proceeding to another room to tell it to the second child, the 'reproducer.' At other times the 'explainer' was immediately asked to repeat the story to the 'reproducer' in the presence of *E*. The 'reproducer' was then asked to give his version of the story to *E*, who next questioned each child separately as to the different items in the story, not for detail so much as for comprehension of the subject-matter.[13] A complete record of the language of the story-telling

[11] *Op. cit.*, 176. [12] *LP*, Chap. 3.

[13] The 'reproducer' was questioned only on the material presented to him by the 'explainer.'

and of the replies to questions was kept. By his technique of questioning, Piaget believed he could find how much more both the 'explainer' and the 'reproducer' understood than they had expressed before the prompting of *E*, and in this way the test became something more than a test of memory.[14]

Our task was to find material fairly hard for and somewhat longer than the immediate verbal memory span of the adults on whom we intended to experiment (college students). We chose selections from articles in the *Scientific Monthly*, on topics with which the students might be vaguely familiar, but not being specialists would find fairly difficult. We used two selections, which we shall designate as A and B. A was on the subject of the effects of climate and was 186 words in length. B was on the subject of the development of astronomy and was 187 words long. Both selections contained 13 essential items to be remembered and understood.[15]

As *Ss* we had 42 women students, freshmen and sophomores at Sarah Lawrence College, whose ages ranged from 17 to 21 yr., with the median at 19. In Series A, 21 *Ss* acted as 'explainers' (X) and the other 21 as 'reproducers' (Y). In Series B, 20 of the Y *Ss* of Series A acted as X and 20 of the X *Ss* of

[14]The stories ranged from 87 to 147 words in length and contained from 9 to 14 items to be remembered or understood, *LP*, 116. Piaget employed explanations of mechanical devices as well as these stories, but since the former involved gesturing as well as verbalizing, we decided not to introduce a factor of which we could not have a record.

[15]The two selections with divisions into items are as follows: Series A. Man's climatic environment affects him in many ways: / His clothing, dwellings, food, occupations and customs; his physical and mental characteristics; his system of government; his migrations; his history—all are affected to a greater or less degree. / The atmosphere is as essentially unalterable as it is all-pervading./ Montesquieu declared that the most enduring of all empires is the empire of climate. Its dominion is invincible. Its laws must be obeyed. It makes no compromise and it grants no pardon. / We may irrigate a portion of the earth's surface and thus remove it partially from the dominion of its local climate, / but we maintain this change only by unending effort. / If left to itself, the desert will encroach upon the irrigated area. / Ancient cities which have been buried in the sands of the desert are evidences of the dominion of climate, / rather than of the destruction accomplished by man. / Man often thinks that he is conquering nature / when he really merely discovers her laws and conforms to them. / We have learned to adjust ourselves to some extent to our climates, / but we cannot change those climates. / (186 words).

Series B. The ultimate object of pure astronomical investigation is to determine the form, motions and physical constitution of our universe, in the broadest sense. / Even its limits are unknown / and no serious scientist would be so bold as to attempt to set them, / any more than he would to the ultimate form which man himself will acquire under the influence of evolution. / Evolution plays its part in the acquisition of knowledge of our universe / as truly as in biology, although of course not in the same sense. / The ancients first occupied their awakening scientific intelligences with the geographical features of their immediate neighborhood / and very naturally concluded that the earth was flat, / which conclusion brought a large crop of difficulties / when they tried to explain the motions of the sun and moon. / When they became more observant and took thought of the apparent forms of the sun and moon and of drops of water, / they were led to the true explanation of a spherical form for the earth as well as for the sun and the moon. / With the invention of the telescope a new era was opened. / (187 words).

Series A acted as Y. The two groups of *Ss* (AX-BY and AY-BX) were equivalent in the scores they made on the Thorndike Intelligence Examination for High School Graduates.[16]

Our procedure was slightly different from that of Piaget, as we were desirous of having graphic records of the repetitions, and yet to avoid oral recitations in the presence of *E*, which, we believe, produced an unfavorable setting. The Xs and Ys, therefore, were each asked to make a written record of what they understood and remembered. The directions to X were:

> This is an experiment to see how well you can repeat to another student what you have remembered and understood of a passage that is read to you. Listen carefully, and when I have finished reading you are to write down what you remember. When you have finished writing you are to go into the next room and read what you have written to your partner.

These directions were given by *E* to Y in modified form to meet the requirements of her task. While Y was writing, X was questioned by *E*. At the end of the experiment Y was in turn questioned by *E* (not in the presence of X).[17]

The experiment was carried on during the spring term of 1931.

Results

We obtained the following numerical results:

	Series A	Series B
Av. no. of items repeated by X to Y	7.471±1.161 (57%)	6.60±1.649 (51%)
Av. no. of items understood by X as a result of questioning	8.524±1.011 (66%)	7.40±1.690 (57%)
Av. no. of items reproduced by Y	4.235±1.08 (57%)	3.70±1.068[18] (55%)
Av. no. of items understood by Y as a result of questioning	4.667±1.057 (62%)	3.90±1.315[19] (59%)

These figures give an estimate of the difficulty of the material for the group of *Ss* in question. They indicate that Y recalled and comprehended the same relative amount of material as did X in each of the two series, but there is a

[16]The average score for the 42 students was 65.4 ± 9.1, range 44 to 80; for the group AX-BY it was 66.8 ± 5.6, range 45 to 80; and for the group BX-AY it was 65.6 ± 6.1, range 44 to 77.

[17]There is no guarantee, of course, that our method of questioning was like that of Piaget. We tried to do as he had done, to ask questions about the memory for and comprehension of the various items, not in a stereotyped manner but quite freely.

[18]The percentages for Y are the proportion of the Y averages to the X averages, not to the total of 13.

[19]We correlated the scores for AX, BX, AY, and BY with the Thorndike scores, and the scores of AX with BY and of AY with BX, by the method of rank differences, translating the ρ obtained into r coefficients by Pearson's correction. The results were low but positive, ranging from +0.25 to +0.41, but with large probable errors (0.14 to 0.15), which was to be expected from the small number of cases.

suggestion that Series B was somewhat more difficult than Series A for both X and Y,[20] a fact to which we shall turn again when we discuss the frequency of the distortions of the materials. The obvious fact that *S*s would recall more after the added stimulus of questioning was also revealed. This was more true of X than of Y, however.

From his averages, Piaget set up four ratios which he considered indicative of the various degrees of comprehension: γ, the number of points comprehended by X divided by the number of points or items to be comprehended; δ, the number of points X repeated to Y divided by the number of points comprehended by X; α, the number of points comprehended by Y divided by the number of points comprehended by X; and β, the number of points comprehended by Y divided by the number of points X repeated to Y. Table I gives these ratios for Piaget's two chronological age levels and for our two series.

TABLE I

RATIOS SHOWING RELATIVE DEGREES OF COMPREHENSION FOR X AND Y

	α	β	γ	δ
Piaget 6-7	.48	.61	.70	.87
Piaget 7-8	.58	.64	.82	.95
Series A	.55	.62	.66	.88
Series B	.53	.59	.57	.89

Except in the case of γ, our ratios for both series seem to be similar to those of Piaget for the 7-8 yr. level, in fact, they are slightly lower. Our γ ratios are decidedly lower, showing that the texts we chose were relatively more difficult to repeat and to comprehend.

Piaget did not claim from his high δ ratios, however, that children understand adults better than they understand each other, for he recognized the fact that the story as originally told was an adult product, prepared ahead in a coherent manner. He limited himself, therefore, to the conclusion that children do not understand each other any better than they understand the adult. We can conclude in a similar manner that our *S*s did not understand each other any better than they understood a logical set-up presented to them.

According to Piaget, the egocentricity of the child is revealed by the fact that he comprehends more than he repeats.[21] Now, in view of the fact that the δ ratios are high (very markedly so at the 7-8 yr. where it is higher than in either of our series), it would seem that this deduction is not entirely justified, and that the children communicated most of the material they understood and remembered.[22] In this respect, the children showed no more egocentrism than did the students, so that we should consider the following conclusion more adequate:

[20]We did not treat the results by reliability formulae, since there were fewer than 25 cases in each of the four groups.

[21]Only in the case of X, Piaget did not give the ratio of repetition to understanding for Y. In our experiment, this ratio was 0.95 for Series A, and 0.91 for Series B.

[22]These ratios were lower in the case of the experiment requiring explanations of mechanical devices. But here the set-up was different; the explanations were made to the children in a manner not so suggestive of a verbal memory test.

depending on the nature and difficulty of the material, the individual shows a limitation of comprehension and of verbal memory even after receiving the added stimulus of suggestive questioning.

In analyzing the records in this particular experiment, Piaget made use of four criteria of egocentricity (as he had previously done with the records of conversations among children); errors of style and grammar, attitude of satisfaction, the presence of juxtapositions and, lastly, the presence of syncretisms; the last two criteria being the ones with which we were chiefly concerned in this investigation.[23]

Juxtapositions at the level of understanding and thinking are recognized,[24] according to Piaget, by the absence of order in the recitation, by causal connection being not expressed but indicated by a simple juxtaposition of terms, by vague explanations in which the why of explanation is omitted, and by the reversal of cause and effect (independent of memory limitations), due to the fact that for the child 'because' does not indicate a univocal link from cause to effect, but rather a vaguer, indifferentiated link (a link of juxtaposition). In the case of the stories, the older children were far less prone than the younger to employ these unsynthesized or 'prelogical' verbal expressions.

Among our *Ss* we found 8 instances (4 made by X *Ss* in each of the two series) of reversals of order in the context.[25] These juxtapositions were not due to momentary oversight, forgetfulness, or "slips of the pen," for, as a result of questioning, the *Ss* still stuck to the reversed order which meant that their original comprehension had been incorrect. For instance, in AX one *S* stated: "Because of the ravages of war rather than the effects of climate, ancient cities are buried in the sands of the desert." The original text read: "Ancient cities which have been buried in the sands of the desert are evidences of the dominion of climate rather than of the destruction accomplished by man." This student did not comprehend the article as a synthetic whole, the main thesis being that the effects of climate are the most powerful factors of destruction in the world. She undoubtedly made use, in interpreting the material, of her past experience about the evil effects of war.

We found only 3 cases (1 each in AX, BX and in AY) where there was juxaposition of statements without connecting link, thus producing discrepancies of logic, and which, upon questioning, the students neither remembered nor comprehended. We give one example, (from AY): "Man has tried to cultivate the land and with the aid of irrigation has succeeded. Man cannot control the climatic conditions that surround him."

There were no instances of absence of order in the recital (at least sufficient to destroy the proper sequence),[26] nor of the wrong use of 'because.'

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[23]The children had had no formal scholastic training, but in the case of the students there could be no way of telling how far correct use of language and grammar was due to a socialized attitude independent of formal training. Although we made no point of investigating the attitude of satisfaction, we heard frequent comments from the students suggesting dissatisfaction with what they had written.

[24]See footnote 2.

[25]Piaget did not give figures for the frequency of juxtapositions and syncretisms.

[26]The students were evidently 'socialized' in the matter of 'order', for they frequently inquired as to its necessity in the written task.

Relatively few of our *S*s (6 in Series A and 5 in Series B) used juxtaposed verbal expressions, but in view of the fact that, despite scholastic training, greater maturity, and more experience in 'socialization,' juxtapositions did creep in, there is still the possibility of assuming that their frequency might be augmented by a further increase in the complexity and difficulty of the presented material.

The reverse phenomenon, that of syncretism, the result of incorporating two or more separate items into an all-inclusive whole, so that the original significance is lost, occurred with greater frequency: 4 times in AX; 4, in AY; 17, in BX (13 different *S*s, 4 having 'syncretized' twice), and 8 times in BY. The students distorted the material far more frequently in Series B than in Series A, and more particularly in BX than in BY. Evidently, the greater difficulty of this series had an effect on the ability to synthesize correctly.

An instance of a high condensation, where the first six items of the text were misconstrued, can be seen in the record of one *S* in BX: "The ultimate astronomical results by astronomers today can be no more ultimate in prediction than what may be the result of evolution in the future, evolution in the world and customs as well as biologically speaking."[27]

From some of the 'syncretic' reports could be traced the set of the individual, her 'egocentric' interpretation. In AY, one *S* was impressed by the statement "man only discovers the laws of nature," and having decided that the theme of the text was that of "man's finding out or discovering," she wrote: "In irrigating a desert we are merely finding out the possibilities of nature." And again she stated: "Just as if we were to find a hidden city buried the thing would have been there always but we are just finding it out." Her X partner had not made this error, but had kept to the original text.

Further 'egocentric' trends are revealed by inventions of subject-matter not given in the original text. These "pure fabrications" did not occur at the 7-8 yr. level, but were more frequently at the younger level, so that Piaget concluded that imagination of this sort was an autistic tendency that disappeared very young.[28] Contrarily, we had a few cases, 3 in A and 3 in B.[29]

If we add together the total number of errors of commission, the juxtapositions, the syncretisms and the inventions, for each of the series, we obtain a better picture of the greater deformation of Series B. In Series A there were 17 distortions and in Series B there were 33.

Discussion and Conclusion

We do not mean to imply that there is anything new or surprising in the fact that material is distorted and transformed when it receives individual interpretation, nor that with greater complexity of material these distortions will occur oftener. The results of myriads of tests of memory, of intelligence, of the reliability of testimony bear witness

[27]See footnote 15.

[28]Piaget meant, of course, "under the conditions of his experiment."

[29]For example, one *S* in AY wrote, "Man believes he can command large industries," and another in AX wrote, "Man may be stranded on a desert with the cool wind blowing across his face."

to this fact. But we have shown that in carrying out an experiment on the memory and understanding of adult students, in a manner similar to Piaget's test procedure, both of administration and treatment, our results approximated his. The amount repeated and understood by 'explainers' and 'reproducers' alike was equivalent to Piaget's 7-8 yr. level, except in the case of the amount of the original text retained by the 'explainers' (γ ratio), suggesting the fact that adults do not understand each other relatively better, nor do they impart relatively more information, than do children in an experiment of this kind.

Furthermore, the adult students as well as the children showed insufficient and exaggerated synthesis of thought by employing verbal expressions of juxtaposition and syncretism, whose frequency varied inversely with increase of age in the case of Piaget, and, in our case, directly with the complexity of the material.

As we have stated previously, Piaget attributes limitation of understanding, of ability to communicate one's thoughts, and the use of distortions of logic to 'egocentrism,' to the incapacity for self-reflection.[30] The breakdown of this egocentric or autistic trend comes about through experience in the social world and by scholastic training as the individual matures, so that in the adult we find considerably more 'socialization,' self-reflection or what Washburn calls "ejective consciousness."

Now, assuming the students to be fairly well 'socialized,' to have developed 'awareness' of their own thoughts and of the thoughts of others, nevertheless, under the conditions of our experiment (where they were presumably free from upsetting conditions and in fairly good 'rapport'),[31] this degree of sophistication did not prevent limited understanding and ability to communicate, or reversion to 'prelogical' or infantile modes of verbal expression.

[30]It has not been our concern in this paper to criticize the use of the terms 'egocentric' and 'socialized.' Two good comments as to their use have been made. Markey says the fact is not so much that the child is egocentric, or believes himself to be the centre of the universe, as that he actually is the center of his known universe; and that the use of the term 'socialized' is "unfortunate on account of the implication that the integration preceding is not social." (*The Symbolic Process and Its Integration in Children*, 1928, 150.) Lorimer has criticized Piaget's use of the term 'egocentric,' as implying too much introversion on the part of the child. (F. Lorimer, *The Growth of Reason*, 1929, 128.)

[31]Six records were rejected because of obvious fatigue or bad humor on the part of the students making them.

Piaget realized the fact that it is not possible either to oppose or to identify the child and the adult,[32] but he did find differences in the 'structure' of thought sufficient to warrant marked comparisons. We have seen that these comparisons hold true only under limited conditions; where the material presented is not difficult for the adult to comprehend, where one child is inferior in chronological age, in ability, or in cultural heritage to another. We feel, therefore, that a more adequate conclusion, and one which would hold true for children and adults of different degrees of maturity, of experience, and of cultural background, would be, that for every normal individual (exclusive of the physically diseased, the mentally pathological and the emotionally unbalanced) there is some compulsory stimulus-situation, requiring coherent or 'logical' thought, which will reveal his limitations of memory, of understanding, of means of communication through verbal exchange; and which will force him into the use of unsynthetic or 'prelogical' modes of thought.[33]

[32]In his article in *Mind*, Piaget says, "Tout ce qui est donné chez l'enfant se retrouve chez l'adulte et tout ce qui est donné chez l'adulte se trouve déjà chez l'enfant." (160).

[33]By this generalization we do not mean to imply that a similar one can be made for the attitudes and beliefs of children and adults, another field in which Piaget has made valuable contributions. We have employed on adults Piaget's method of recording spontaneous conversations over a period of time, in order to investigate attitudes and interests, but the work is not sufficiently advanced as yet to predict results.

A QUANTITATIVE ANALYSIS OF INDIAN MUSIC

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In contrast to the conceptual nature of poetry and literature and to the representative character of the plastic arts, music is often considered an immediate language of the emotions. Many have claimed that music produces its effects 'naturally' even upon untrained auditors.[1] So far as Western music is concerned, however, there stands against this view the recent studies showing the effects of practice on preferences for various musical endings, and on the emotional quality of various types of chord.[2] These studies indicate that previous experience and training may play very important rôles in our appreciation of music. Individuals holding a generalization of such views consider most aesthetic preferences as dependent upon the observer's learned emotional responses to the elements of the artistic object. Since this experimental work is too recent to be assimilated into aesthetic theory, the arguments used by relativists against the generalizations of classic aestheticians are seldom based upon experimental evidence. The arguments usually cited may be summarized as follows; The 'canons of art' are inconstant, holding neither for different periods of time nor for different individuals at the same time; the origins of many preferences can readily be explained on the basis of learning, as many of the psychoanalytic interpretations point out; and finally, with particular respect to music, there are marked differences in the scales of different peoples as well as in the type of music developed with their use, although the strangest music may become, with sufficient repetition, a source of great enjoyment.

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[1]See, for example, R. Wallaschek, *Primitive Music*, 1893, 1-326; J. Combarieu, *Music: Its Laws and Evolution*, 1910, 164-175; A. Balz, Music and emotion, *J. Philos.*, 11, 1914, 236-244; F. Densmore, The rhythm of Sioux and Chippewa music, *Art and Arch.*, 9, 1920, 59-67; H. Roberts, Study of primitive music, *Amer. Anthropol.*, 24, 1922, 144-160; G. Wilkinson and A. Gray, *The Mechanism of the Cochlea*, 1924, 188 ff.

[2]P. R. Farnsworth, Ending preferences in two musical situations, this JOURNAL 37, 1926, 237-240; Ending preferences among the three positions of the tonic chord, *J. Comp. Psychol.*, 6, 1926, 95-102; Concerning art standards, *Psychol. Rev.*, 33, 1926, 324-328; C. M. Heinlein, The affective characters of the major and minor modes in music, *J. Comp. Psychol.*, 8, 1928, 101-142.

Such suggestive evidence calls for an experimental attack upon a problem which has been noted frequently enough, and which has enlisted some meager efforts towards solution. Myers long ago declared that to "investigate the degree of universality of those signs in verbal and musical language which serve to communicate states of feeling, is a matter of no small interest." "We would know," he continues, "how far the modification of verbal language in respect to pitch, loudness, timbre and tempo serve as universal methods of communication, and similarly how far the feelings of sorrow, joy and the like, which a given piece of music evokes in the community that produced it, are shared by members of other communities more or less advanced in civilization."[3] Much later Diserens added the comment that "the precise influence of different modes and types of music has not been determined, and waits upon an adequate classification of musical selections, which must probably proceed at first by introspective and statistical methods."[4] Only tentative steps have been made in the direction of such an analysis. Balz offers observations on the differences in martial, dance, and operatic music.[5] Densmore, on the supposition that the music of a race is an expression of its life, has analyzed the rhythm and melody of a few Indian songs.[6] There seems, however, to have been but one statistical study, that of Watt, who tabulated the various intervals in Schubert's songs and compared them with tabulations of the intervals in the songs of the Chippewa and Teton-Sioux.[7] Watt found that, for all types of song, the average number of any particular interval was practically the same from both sources.

Most of the experimental work in music, however, has centered about two other problems. The first—the studies of the physiological and behavioral effects of tones, chords, and music—has been fully though uncritically reviewed by Diserens.[8] The second includes the studies of musical enjoyment. These studies indicate that the experimenters have often assumed, in their selection and characterization (as of 'sad' and 'gay' pieces) the very knowledge the present paper attempts in part to determine.[9]

Problem

The problem of the present paper is to determine whether certain objective characteristics of a piece of music form the basis for the

[3]C. S. Myers, Ethnological study of music, *Anthrop. Essays Presented to E. B. Tylor*, 1907, 236-7.

[4]C. M. Diserens, Reactions to musical stimuli, *Psychol. Bull.*, 20, 1923, 173-197.

[5]A. Balz, *op. cit.* [6]F. Densmore, *op. cit.*

[7]H. J. Watt, Functions of the size of interval in songs of Schubert and of Chippewa and Teton-Sioux Indians, *Brit. J. Psychol.*, 14, 1924, 370-386.

[8]C. M. Diserens, *The Influence of Music on Behavior*, 1926, 1-224. Here is summarized the folk-lore on the effects of music on animals, on the sick, on workers and on its use in myth and magic. The book concludes with the history of the experimental studies, ending with a study by the author himself.

[9]Cf., for example, B. Gilman, Musical expressiveness, this JOURNAL, 4, 1892, 558-576; 5, 1893, 42-73; J. Downey, A musical experiment, *ibid.*, 9, 1897, 62-69; C. S. Myers and C. W. Valentine, A study of individual differences in attitude towards tones, *Brit. J. Psychol.*, 7, 1913, 68-111; C. S. Myers, Individual differences in listening to music, *ibid.*, 13, 1922, 52-71; H. P. Weld, An experimental study of musical enjoyment, this JOURNAL, 23, 1912, 245-308; and most of the studies in *The Effect of Music*, (M. Schoen, ed.) 1927, 1-273.

mood which it may arouse; and whether these objective characteristics are fairly universal, or are largely dependent upon local musical traditions.

Method

The method by which this problem is attacked has passed through a number of phases. (1) A direct attack upon Western European music seemed out of the question; this music has tremendous complexity and it offers no obvious basis for classification according to mood; it is divorced from any important rôle in daily life; and it alone in the world of music is structurally harmonic in fact. The type of music common today is said to have originated in the fifteenth century. Any generalizations based upon a more primitive music, however, must be tested upon our Western varieties.

(2) An examination of folk-music again meets great difficulties. There are many ballads in which some simple melody is repeated for an indefinite number of verses, verses which tell a story appealing to a number of contrasting emotional attitudes. Non-ballads, on the other hand, very often refer to characteristics of a people or neighborhood not shared by other folk groups.

(3) The music finally subjected to analysis has been selected from records made of North American Indian tribes. The Indian songs have the advantage, for our purposes, of being closely connected with important aspects of tribal life, and are hence sung upon definite occasions. In consequence they fit more nearly a single emotional attitude. This fact is further attested by the brevity of most songs, and their maintenance of a fairly uniform character. Again, the songs of a number of tribes have been transcribed and published; this allows a check of the reliability of the results among peoples of the same general cultural level. After the development of standards of classification and comparison with the Indian songs, a group of folk songs were analyzed for comparison.

The Indian songs used have been taken from the Chippewa (118 songs), the Teton division of the Dakota Sioux (83 songs), the Mandan and Hidatsas (35 songs), the Northern Utes (31 songs), the Papagos (36 songs), and the Ojibways (31 songs). All save the songs from the last-named tribe were collected by Frances Densmore.[10] Burton's collection of Ojibway songs[11] includes practically none but love songs; and frequently he omits the metronomic rate or even a general indication of the speed of the song. This is not as bad, for our purposes, as Gilman's little collection of Zuni songs[12] which give not the slightest indication of what they are about or when they are sung. The folk songs used in comparison include representatives of Irish, English, Welsh, Scottish, Swedish, German, Italian, Slavic and Asiatic Indian songs.[13]

The selected songs have been subjected to a fairly complete analysis. The major classification of the songs into groups having fairly constant emotional tone was made largely on the basis of the situation in which the song was sung.

[10]The collections are published by the *Smithsonian Institution, Bureau Amer. Ethnol.*, Vols. 45, 53, 61, 75, 80, and 90.

[11]F. R. Burton, *American Primitive Music*, 1909, 1-284.

[12]B. Gilman, Zuni melodies, *J. Amer. Ethnol. & Archaeol.*, 1, 1891, 65-91.

[13]Twelve songs taken from *Irish Country Songs* (H. Hughes, Ed.), 1909; 8 from *Gems of Welsh Melody* (J. Owen, Ed.), 1886; 31 from *One Hundred Folk Songs* (H. F. Gilbert, Ed.), 1910.

There are three major classes; healing the sick or injured, love songs, and war songs. The healing songs form one group. The love songs are further divided into three groups; 'sad' (parting or absence of lover, death of lover), 'gay' (merry-making, trysts), and the remaining, unspecified songs. The war songs are divided according to the stage of war-like activity which the songs accompany: preparation in camp, on the war path, scouting, in battle, victory (and scalp dances), and the recital of great deeds. War-medicine songs are usually included with the war songs, and the love-medicine songs with the love songs. The various game, dance, gambling and parade songs are omitted from treatment. These various groups of songs are then compared with respect to their pitch, range, speed, types of interval, and types of rhythm. Only the treatment of the intervals and of the rhythms needs comment. Intervals were counted under three heads; those larger than a third, thirds (major and minor), and those smaller than a third. The divisions were made large enough to eliminate any errors in the transcription of the intervals into divisions of Western European music. The correct intervals are probably not always accessible. The divisions used are unsatisfactory in two respects. There should be a separate division for notes sung on the same tone, and there should be a separate division for fourths. A great deal of difficulty was encountered in making classifications of the rhythms. No general scheme, repeated throughout, such as is found in *My Country 'Tis of Thee* occurred, except in the rarest instances. Although a scansion of the songs with similar titles showed certain general resemblances, these could not be stated. The common unit of the rhythmic scheme had to be reduced, finally, to the extent of a measure or less. The percentage of recurrence of each rhythmic unit which was repeated enough to occupy at least one tenth of the song was recorded. A tabulation of these rhythms gave so many divergent patterns that it took several pages to indicate them all. A utilization of those units that occupied at least 20% of the song, on the other hand, would have cut out very many significant occurrences. The tables on rhythms here presented, finally, are based upon the incidence of the short phrases (about 35) each of which constituted at least 15% of the song. It is thus possible for a song to be represented in 6 types of rhythm ($6 \times 15 = 90$), or in none.

An indication of the reliability of the differences based on data from the various tribes may be obtained by comparing the variations of each tribe with that of the entire collection. Since the tribes are not represented by an equal number of songs in each category, differences shown by composite tables could be due simply to the peculiarities of the music of the tribe most represented. Tribal variations will be indicated as the data are discussed.

Results

Table I presents a classification of the songs from each tribe according to the various situations selected as emotionally homogeneous.

Table II presents, by classes, the data for range, pitch, and speed of the songs. Many of the differences are statistically significant, although none of the differences are great enough to form the basis of any successful predicting. About 25 of the differences are large

TABLE I

THE NUMBER OF SONGS FROM THE SEVERAL TRIBES REPRESENTED IN EACH CLASS

	Chip. I	Chip. II	Teton-Sioux	Mandan Hidatsa	Ute	Ojibway	Papago	Total
War-songs								
Organization	11	13	18	7	2	1	1	53
War-path	—	7	8	3	—	1	4	23
General	10	6	2	3	5	—	—	26
Scout	—	1	—	2	2	1	—	6
In battle	1	4	3	3	—	—	2	13
Victory	7	10	15	8	7	—	15	62
Recitatives	2	4	7	1	—	2	—	16
Healing songs	8	—	15	—	15	—	11	49
Love songs								
Sad	9	7	7	2	—	6	1	32
Happy	5	5	1	3	—	13	—	27
Other	5	1	2	1	—	7	—	16
Miscellaneous	—	2	5	2	—	—	2	11
Total	58	60	83	35	31	31	36	334

TABLE II

THE AVERAGE RANGE, DEPTH, AND SPEED OF EACH CLASS OF SONG

	Range			Pitch of lowest note*			Speed†		
	Mean	σ	σ_m	Mean	σ	σ_m	Mean	σ	σ_m
War songs									
Organiz.	10.61	1.73	.24	4.00	2.93	.40	91.5	16.87	2.34
War-path	8.85	1.68	.35	5.80	3.65	.76	85.5	16.67	3.55
General	10.27	1.61	.32	3.81	3.10	.61	95.8	20.20	3.96
Scout	8.83	1.86	.26	4.60	2.22	.91	79.0	22.45	3.58
In battle	9.92	1.94	.54	4.54	1.98	.55	73.5	15.62	4.33
Victory	9.00	1.88	.24	4.52	2.80	.36	85.9	19.70	2.50
Recitatives	9.53	1.41	.35	6.23	2.64	.66	83.7	17.46	4.67
Healing songs	8.28	2.07	.27	5.96	3.03	.43	80.0	19.85	2.86
Love songs									
Sad	9.62	1.77	.31	5.26	3.66	.65	75.0	15.18	2.87
Happy	9.50	2.14	.41	7.79	4.27	.82	80.0	13.50	2.70
Other	9.93	2.04	.51	6.25	3.20	.80	77.8	19.82	5.55
Av.	9.46			5.30			84.		

*Substitute pitch A for 1, A♯ for 2, B for 3, etc.

†The number of songs represented in certain of these figures is reduced due to the fact that the rate for a number of the Ojibway songs was not given.

enough to occur by chance once in a thousand random samplings (where the difference between the means is at least 3.08 times the standard error). A few of the D/σ values are:

Organization *vs.* Healing: for range, 6.47; pitch, 3.32; speed, 3.11;
General war *vs.* Healing: for range, 4.76; (pitch 2.75), speed, 3.90;
Organization *vs.* Sad Love: for pitch, 3.75; speed, 4.45;
Organization *vs.* Gay Love: for pitch, 4.12; speed, 3.22;
General war *vs.* Gay Love: for pitch 3.90; for speed, 3.29;
Organization *vs.* Scout: for range 5.05.

TABLE III

THE PROPORTIONAL FREQUENCY OF THE DIFFERENT INTERVALS, AND THE TYPES OF RHYTHM FOUND IN THE VARIOUS CLASSES OF SONG

	Type of interval (percent)			Reliability of intervals	Type of rhythm* (percent)		
	'large'	'thirds'	'small'	(by tribes)	even	uneven	rough
War-Songs							
Organization	7	22	71	2.31	30	30	40
War path	8	17	75	4.80	23	41	36
General war	8	19	73	6.11	28	39	33
Scout	14	17	69	5.27	0	31	69
In battle	11	23	66	3.83	16	22	62
Victory	8	21	70	3.79	25	24	51
Recitatives	6	19	75	3.82	31	23	46
Healing songs	9	14	77	3.31	40	40	19
Love Songs							
Sad	15	20	65	6.50	42	43	14
Happy	9	20	71	3.43	39	32	29
Other love	15	20	65	2.77	27	50	22
Av.	9	19	71	5.22	31	34	35

*See Table IV for examples of the various rhythmic units.

The Chippewa in general sing with a wider range, greater speed and depth than the other Indians; but in most cases the mean for each class of Chippewa songs varies in parallel with that of the rest of the tribes. We can conclude that organization and general war songs are low, rapid, and of wide range. By contrast the love songs tend to be high, slow, and of medium range. Healing songs have a very narrow range, are moderately slow, moderately high. The higher pitch of the love songs is not due to the fact that some women sang, for in those cases the pitch was recorded an octave lower.

Table III presents the general data for types of interval and types of rhythm. The reliability of the types of interval has been measured in two ways. The first is a measure of consistency. The percentages

of the intervals for every other song from a random arrangement of each group were averaged and compared with the average percentages for each entire group. In only 4 of the classes of song were the differences for any type of interval as large as 3–4%; these 4 are scout songs, war path, happy love, and the miscellaneous love songs. The second method consisted in using a fairly elaborate device. A table was made giving the percentages of types of interval in each class of song for each tribe. (Where two tribes had only one or two songs in a given class, these were combined.) A composite distribution of the differences of these values from the grand average of 'large,' 'thirds,' and 'small' intervals for all of the songs was made. The σ of this distribution was 5.22. Next, a similar distribution was made for the differences of the songs by tribes in each class. Thus, Chippewa organization songs have 4% 'large,' 22% 'thirds,' and 74% 'small' intervals. The first and last values each differ by 3% from the average song of organization. The values of these σ are presented in the fourth column of Table III. Two values are less than half that for the entire group, and three exceed this value. The low validity of the Scout songs is due to the low value of large intervals in the Chippewa and the Ojibway samples. In the general war songs the Chippewa have but 4% 'large' intervals, as opposed to 18% for the Utes; the Teton-Sioux, and the Mandan-Hidatsa have but 10% 'thirds,' while the Chippewa have 23%. In the love songs the Chippewa and the Ojibway have many 'large' intervals when the songs are sad (19% and 22%), and few when the songs are happy (8% and 7%); but the Teton-Sioux have just the reverse (7% 'large' in sad, 16% 'large' in happy love songs).

The data for differences in types of rhythm are the most striking and significant that we have to deal with. The degree of rhythmic staccato or punctuation in the songs seems highly correlated with the degree of excitement represented by the situation. Comparing the three major groups of song with respect to the percentage of rough rhythms, it appears that the healing songs have fewest, the love songs next, and the war songs most. An inspection of the values for the even rhythms shows just the reverse. The correlation holds further within the members of these major groups. The happy love songs have more rough and fewer even rhythms than the sad love songs. The scout, battle, and victory songs have a majority of rough rhythms, while the scout and battle songs have the fewest even rhythms.

An interesting comparison can be made between the healing and the sad love songs. These two groups are practically the same with respect to the proportion of rhythmic units. But they differ markedly so far as types of interval are concerned. While the healing songs have the greatest number of small intervals, the sad love songs have the fewest of any of the classes; and while the healing songs have the fewest number of thirds, the sad love songs have slightly more than the average.

Tables IV and V present certain additional information about the rhythmic units and the incidence of extreme values in percentage of intervals. From Table V it may be seen that nearly a third of the songs composed of 85% or more of very small intervals are healing songs. Of the remaining 27 songs in this category, 11 are charm songs. The organization of war parties, the victory songs, and the songs in battle contribute half or more of their songs represented in this table to the group having a large percentage of thirds. The greatest contribution to the songs having a high percentage of large intervals comes from the sad and unclassified love songs, and, strangely enough, from the victory songs.

The complex facts relative to the general classification of the Indian songs in the various items of comparison are brought together and summarized in Table VI. Here the war songs are classed into two or three groups, and the love songs into one or two groups, depending upon the closeness of agreement of the members. A total picture of the songs warns us that no particular item can be picked out and labeled as the typical characteristic of a certain class of songs. One can readily see that the characteristics analyzed in this paper are not component parts of the songs, but aspects holding simultaneously for every phrase of a song; and that the effect of one or another characteristic depends not simply upon itself, but also upon the other characteristics. The rhythmic units composing sad love and healing songs are quite comparable in isolation. But the emphasis in the healing songs upon small intervals, narrow range, and greater speed makes the rhythmic structures seem quite different. Again, the rhythmic analysis of the gay love and the first group of war songs (organization, war path, general), and the analysis of the types of interval would indicate that they were highly similar songs. But the greater speed, range and depth of the war group changes even the apparent character of the analytically similar aspects. Of course the

words, facial expressions, gestures, intonation, and loudness of the song as performed are additional factors of importance which cannot be taken into this account at all.

TABLE IV

DISTRIBUTION OF CASES OF TYPICAL RHYTHMIC UNITS

	Songs of										
	War							Healing	Love		
	Organiz.	War path	General	Scout	Battle	Victory	Recitatives		Sad	Happy	Other
Even*											
22-22	12	4	3	0	1	10	0	15	4	0	4
8	7	3	5	0	2	15	7	24	13	8	4
44 (4)	10	4	8	0	2	6	3	13	9	9	2
88	0	0	1	0	0	1	0	5	3	3	1
Other	4	1	2	0	0	4	1	0	0	3	0
Total	33	12	19	0	5	36	11	57	29	23	11
Uneven*											
62	1	1	3	0	0	2	2	11	6	2	7
624	0	2	3	0	0	2	1	4	4	1	1
422	5	5	5	1	0	6	2	11	5	3	2
224	7	3	6	1	1	8	0	8	3	3	0
4222-2224	2	1	2	1	1	2	0	9	0	2	0
84	2	1	4	0	0	2	0	2	0	0	0
Other	16	9	4	1	5	14	3	13	10	8	10
Total	33	22	27	4	7	36	8	56	28	19	20
Rough*											
314	9	2	3	1	0	7	4	3	2	2	1
431	10	3	0	1	6	7	0	8	1	5	0
31-211	3	1	3	0	0	9	0	3	0	0	1
31-31	5	3	2	1	2	6	1	2	0	0	0
22-31	1	1	2	1	1	5	1	1	0	2	0
13-31	1	4	1	0	1	6	1	0	0	1	1
Other	15	5	12	5	10	32	9	9	7	7	6
Total	44	19	23	9	20	72	16	26	10	17	9
Grand Total	110	53	69	13	32	144	35	139	69	59	40

*Rhythm expressed in musical notation, in 16ths., *i.e.* 1 equals a sixteenth; 2, an eighth; 3, a dotted eighth; 4, a quarter; 6, a dotted quarter; and 8, a half note.

There are a number of other factors which tend to decrease the reliability of what differences actually exist. Even though we assume that the songs are all properly classified, it does not follow that every song in each class typifies or expresses the mood of that situation

TABLE V

THE OCCURRENCE OF SONGS WITH EXTREME VALUES OF INTERVALS

	Songs of War: Organiz.	War path & Gen.	Scout	Battle	Victory	Recit.	Healing	Love: Sad	Happy	Other	Total
85% intervals less than third	7	8	0	1	4	2	11	1	3	0	37
33% intervals thirds	11	8	1	2	14	1	1	4	4	0	46
20% intervals greater than thirds	3	3	2	1	9	0	4	12	4	5	43
Total occurring	21	19	3	4	27	3	16	17	11	5	126
Proportion of all songs of this class	40	41	50	31	44	19	33	53	41	31	39

TABLE VI

SUMMARY OF THE GENERAL DATA ON INDIAN MUSIC IN TERMS OF RANK ORDER

Class		Range	Pitch	Speed	Type of interval: 'large'	'third'	'small'	Type of rhythm: even	uneven	rough
War	Organization war path general	5	2	5	1	3	4	2	3	4
War	recitative victory	4	4	4	1	3	4	2	1	5
War	scout battle	2	1	2	4	2 5	2	1	1	5
	Healing the sick and wounded	1	5	3	2.5	1	5	4	4	2
Love	Sad	3	3	1	5	4	1	5	5	1
Love	Gay		6		2.5		3	3	2	3

The rank order 1 always indicates the class with the smallest value: low range, low pitch, slow, and small percentages of types of interval and rhythm.

equally well. The old and established tribal songs that have managed to survive several generations might best represent the typical characteristics of a song for a given mood; but there are only a few songs known to be old. Our groups actually contain many that are 'personal' and a number that are relatively new. A second factor that decreases reliability is the difference in attitude of the various tribes upon the same objective situation. For such people as the Chippewa or Teton-Sioux, a war expedition was often a sort of game in which horses, or a few safely taken scalps, were the stakes. The Papagos were, on the other hand, very pacific, and never went to war against their enemies the Apaches except in retaliation. The expeditions were

TABLE VII

TYPES OF INTERVAL AND RHYTHM FOR SOME SPECIAL GROUPS

Class	No.	Type of interval (percent)			Types of rhythm (percent)		
		'large'	'thirds'	'small'	even	uneven	rough
Derision	7	8	20	72	36	36	27
War-medicine	13	5	9	86	44	38	19
After killing a warrior	8	7	24	69	13	7	80
Death songs	7	12	17	71	20	40	40
Scout songs	6	14	17	69	0	31	69
Disappointment in love	5	32	11	57	14	57	29
Lonesome	8	21	19	60	31	38	31
Parting	5	16	17	67	40	60	0
Absence	7	14	26	60	26	42	32
Death of lover	11	8	18	74	40	36	24
Happy love	14	12	23	65	20	56	24

well organized crusades; a definite routine of activity was required especially on the part of the medicine men. The warriors themselves had to do certain things on the various nights out from home, including the recital, by members of an appropriate group, of a pair of set speeches. Upon completion of the expedition those warriors who had killed Apaches were required to undergo a 16-day ordeal of purification, involving hardships even greater than those of the warpath. It was considered a crime to kill even an enemy, punishable by the spirits of the dead. Another factor is the breadth of the categories used in classifying the songs. In order to have enough cases to make the differences statistically significant, it has been necessary to include more and more dissimilar songs. But now that certain types of difference seem to stand out as significant, for the total groups of songs,

differences of the same character for small groups of highly similar songs will have more significance. Table VII presents the data with respect to intervals and rhythms for 11 such small, but homogeneous, groups. A similar set of 10 groups has been prepared from an analysis of the folk songs (Table VIII). This last list is very unsatisfactory

TABLE VIII

TYPES OF INTERVAL AND RHYTHM FOR SOME GROUPS OF EUROPEAN FOLK SONGS

Class	No.	Types of interval (percent)			Types of rhythm (percent)		
		'large'	'thirds'	'small'	even	uneven	rough
Prep. for war	5	10	15	75	20	60	20
War marches	5	7	18	75	10	30	60
Victory songs	2	11	32	64	25	0	75
War dirge	2	12	17	71	50	50	0
Death of lover	6	12	17	71	11	78	11
Absence, parting	4	9	15	76	37	50	12
Lonesome, sad	9	12	11	77	56	44	0
Description of, or song to, love	17	9	16	74	22	69	9
Sentimental or serious love	7	6	9	85	50	43	7
Gay, playful	4	13	17	70	57	43	0

since there are less than 6 songs represented in more than half the groups. The comparisons that can be drawn are obviously only suggestive. The outstanding characteristics of the data are readily seen. It likewise appears that the agreement between the Indian songs and the folk songs extends only so far as the major classes dedicated to war and love.

CONCLUSION

In spite of all these factors of unreliability, the limitation in the completeness of analysis, and the limitations of analysis itself, fairly straightforward and significant differences stand out between certain classes of songs which doubtless have a different emotional appeal.

In conclusion, it appears that at least the dynamic characteristics of moods can be communicated by song even when the performers or audience have no means of conceptual communication. The dependence of this communication through the common effects of rhythms and tempo upon bodily movements seems apparent; but no such simple explanation seems applicable to the results with respect to

distribution of intervals, or of the range or depth of the song. The particular significance of various types of rhythm and interval, range, pitch, depth, and their combinations for one mood or another cannot be clearly and easily deduced from this Indian music since the attitudes and feelings of such peoples in the classified situations are not readily determinable. A determination of the significance of these aspects of music could be made, as it seems to me, on the basis of a statistical analysis such as the one here presented of a body of Western European music whose expression of a particular emotional tone had actually been established by the agreement of a number of auditors.

THE MEASUREMENT OF MUSCULAR TENSION AS A TECHNIQUE FOR THE STUDY OF EMOTIONAL TENDENCIES

By Elizabeth Duffy, Sarah Lawrence College

Experimental investigation of the 'emotions' has shown almost uniform neglect of the highly significant changes in the skeletal muscles which occur under 'emotional' stimulation.[1] Although one of the most prominent theories of the emotions maintains that the prime function of the bodily changes in certain emotional states is to energize the muscles,[2] few investigators have studied directly the relation between skeletal muscle changes and emotional states. Indeed, many investigators, when observing such muscular changes during emotions, have appeared to regard them as entirely incidental and in no way an index of the state itself.

There are, however, a few studies which have been concerned with this problem. Golla has reported an increase in tonus of the arm muscles upon presentation of a 'nocuous' physical stimulus.[3] Johnson has obtained measures of tension in the muscles of the hand during a tapping performance, and in presenting her results has advanced the theory that "temperamental differences are indicated by differences in tension of expressive muscles under differing stimuli."[4] Jacobson has suggested,[5] as a result of experiments with normal and neurotic subjects, that the 'curve of relaxation' (the diminution of the knee jerk during a period of experimentation) is a possible test for nervous hypertension or neurosis. Duffy has found,[6] in an experiment with nursery school children, that individual differences in excitability were definitely correlated with differences in tension in the

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[1]Thanks are gratefully extended to the Committee on Child Development of the National Research Council for the fellowship which made this investigation possible; to the Child Development Institute of Teachers College, Columbia University for the generous extension of its facilities; to the Department of Psychology of Columbia University for appropriations for apparatus; and to Professor R. S. Woodworth for advice and coöperation.

[2]W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 1916, 215-216.

[3]F. L. Golla, The objective study of neurosis, *Lancet*, 201, 1921, 218.

[4]Buford Johnson, Changes in muscular tension in coördinated hand movements, *J. Ex. Psychol.*, 11, 1928, 334.

[5]Edmund Jacobson, Differential relaxation during reading, writing, and other activities as tested by the knee-jerk, *Amer. J. Physiol.*, 86, 1928, 693.

[6]Elizabeth Duffy, Tensions and emotional factors in reaction, *Genet. Psychol. Monog.*, 7, 1930 (no. 37), 65.

muscles of the unused hand when the subjects were making discriminative reactions. Luria[7] has reported a study of motor reactions during certain associative processes and has found that the disturbances of the motor curve (obtained by pressing with a finger on a pneumatic appliance) expressed a corresponding affective change evoked by the verbal reactions. A few other investigators, including Morgan[8] and Freeman,[9] have reported increases in muscular tension in situations which might be, but were not usually, classed as emotional.

Experiments in this field are obviously few in number. Of the available studies only one or two have attempted to demonstrate that individual differences in the emotional tendencies of normal subjects may be revealed through differences in tension of the skeletal muscles. Because of the importance of the question it seemed desirable to obtain more conclusive evidence, and the present experiment was undertaken as an extension of the writer's earlier investigation. In this study we obtained measures of tension in the muscles of both the used and the unused hand while our *S*s, 18 nursery school children, were making discriminative reactions to pictures, and again while they were tapping. These measures of tension were related to estimates of excitability and of degree of adjustment to the nursery school. Tension during discriminative reactions was measured for every *S* on 5 separate occasions, affording data on the effect of habituation upon tension, and on the constancy of individual differences in muscular tension.

The plan of the study was analytic and exploratory, emphasizing intensive work with a small number of *S*s. It seems reasonable to suppose, however, that a high degree of consistency of results, when considered from a number of different points of view, indicates reliability of the data comparable to the reliability obtained from a single variety of data based on a larger number of *S*s.

Experimental Procedure

The *Ss* were 18 children in the Child Development Institute of Teachers College, Columbia University.[10] Their ages ranged from 2 yr., 11 mo. to 3 yr., 10 mo. (median, 3 yr., 3 mo.)

[7]A. R. Luria, Die Methode der abbildenden Motorik in der Tatbestandsdiagnostik, *Zsch. f. angew. Psychol.*, 35, 1930, 139-183.

[8]J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 35, 1916, 42-48.

[9]G. L. Freeman, Changes in tonus during completed and interrupted mental work, *J. General Psychol.*, 4, 1930, 309-332.

[10]Two of the *Ss* failed to comply with the instructions for the discriminative reactions. Their records were discarded. One *S* could not be obtained for the tapping test. The results for discriminative reactions are based on 16 *Ss*, and for tapping, on 17 *Ss*.

(*1*) *Discriminative reactions.* The *Ss* were brought to the laboratory individually and seated at a small table in front of a screen on which an automatic lantern projected a series of 40 pictures at precisely 4-sec. intervals. The room was slightly darkened and *S* was instructed to press a key, which sounded a buzzer, whenever a picture of an automobile appeared, but to make no response to other pictures, which were of a horse and of a street car.[11] The slides were presented in approximately random order.[12] The appearance of the pictures and the closing of the key were graphically recorded. The degree of pressure upon the reaction-key was also graphically recorded by a specially constructed device.[13]

Throughout the experiments *S* held in each hand a dynamograph, which gave a graphic record of the pressure of his grip.[14] A large stylus attached to the dynamograph held in the right hand was used for closing the reaction-key. During the interval between successive reactions the stylus was rested upon the base of the key. Arm supports prevented strain. Temperature, light, and sound were held as constant as was feasible. All experiments were conducted between 9:30–11:00 A.M.

After 2 preliminary practice series this experiment was repeated 5 times with every *S* within a period of 4 mo. The order of the pictures was altered for every presentation. The last 3 series of the experiment differed from the first 2 in two respects: (a) a red light was substituted for the buzzer, and (b) between reactions the stylus was rested on a fixed support instead of the key-base.[15]

(*2*) *Tapping.* After the *Ss* had completed the discriminative reaction experiments, they were given the tapping test, which consisted of tapping as rapidly as possible, first for 15 sec. with the right hand, and then for 15 sec. with the left hand. The stylus for tapping was the one attached to the dynamograph for the right hand and previously used for closing the reaction-key. In the unused hand the *S* held the dynamograph held in this hand during the discriminative reactions. Graphic records were obtained of the pressure from the unused hand and the pressure from the used hand, together with 1-sec. time intervals.

Results

(*1*) *Individual differences.* Our results reveal marked differences between individuals in the tension of their muscles as that was

[11]The picture of the automobile showed a child crossing the street directly in the path of the car.

[12]Not more than 3 pictures of one kind were presented in succession.

[13]The reaction-key rested on a movable base calibrated for pressure in 50 grm. increments from 150 grm. (the weight of the dynamograph) to 850 grm.

[14]The dynamographs were made of rubber tubing (8 × 1 × 1/8 in.) plugged at both ends with an aluminum drum. They were connected with an air system at atmospheric pressure and were calibrated in mm. of mercury from 0–210 mm., by 10 mm. increments.

[15]The first change was made because the buzzer proved unpleasant to several *Ss*. The second because pressure during the rest period obscured the record of pressure upon the reaction-key. Only pressure records obtained after this change (Series III, IV, and V) could be used in the results.

measured by the pressure of the hand upon the dynamograph.[16] Table I gives the average tension scores, with their m.v., and the range of dispersion, for each hand for the discriminative reactions and the tapping. The average tension during the discriminative reactions for the unused hand was 16.8 ± 2.1 mm., ranging for the different *S*s from 12.6–21.4 mm.; and for the used hand it was 23.2 ± 3.6 mm., ranging from 17.2–33.8 mm. The average tension during the tapping was: for the unused hand 18.9 ± 4.5 mm., range, 10.0–30.8 mm.; for the used hand 40.3 ± 13.1 mm., range 20.0–73.6 mm.

TABLE I

SHOWING THE AVERAGE TENSION SCORES WITH THEIR M.V., AND THE RANGE IN EACH TEST

(Values in mm. of mercury)

Test	Hand	Av.	M.V.	Range		
				high	low	diff.
Discrimination	unused	16.8	2.1	21.4	12.6	8.8
	used	23.2	3.6	33.8	17.2	16.6
Tapping	unused	18.9	4.5	30.8	10.0	20.8
	used	40.3	13.1	73.6	20.0	53.6

Three questions arise concerning the results. (1) How constant are the individual differences? (2) What is the psychological significance of the differences? and (3) What, within the limits of this experiment, is the best measure of tension?

(*a*) *Constancy during successive performances.* The discriminative experiments, it will be recalled, were given the *S*s 5 times over a period of 4 mo. The data obtained afford information concerning the constancy of the individual differences in tension.

In comparing the tension scores made in these 5 separate experiments there are two disturbing factors which must be taken into account: (1) the change in apparatus and procedure after Series II, which partially invalidates a comparison of Series I and II with the later series; and (2) the fact that 9 of the 16 *S*s developed whooping cough during the period when Series III and IV were being recorded. This suggests that the reliability of these records may have been de-

[16] A tension score was obtained for the highest point reached by the pressure tracing during every 4-sec. interval. An *S*'s score for each hand during a given series of the experiment represents the average of 40 such scores. His final score for each hand during the 5 series of discriminative reactions represents an average of 200 such scores.

creased through the effects of illness and absence. Table II gives the coefficients of correlation[17] for the tension in the used hand[18] during the 5 experiments (Series I–V).

TABLE II

COEFFICIENTS OF CORRELATION FOR TENSION IN THE USED HAND DURING DISCRIMINATIVE REACTIONS

Series	I	II	III	IV
II	.74 P.E. .08			
III	.24 P.E. .17	.21 P.E. .17		
IV	.33 P.E. .16	.21 P.E. .17	.45 P.E. 14	
V	.58 P.E. .12	.33 P.E. .16	.34 P.E. .16	.74 P.E. .08

There are two instances of a close correlation between series: that of Series I with Series II ($r = 0.74$) and that of Series IV with Series V ($r = 0.74$). Other coefficients of correlation range from 0.21–0.58. Table IV shows that the series which correlate most closely had the smallest median number of days between them. But not only was this true, they had also (with the exception of Series IV) the smallest number of *S*s absent from the nursery school for excessively long periods before the taking of their records. When Series III was recorded, 5 of the *S*s had taken no part in the experiment for 57 or more days, and when Series IV was recorded, 5 of the *S*s had taken no part for 42 or more days. When Series I and II were recorded the epidemic of whooping cough had not occurred, and when Series V was recorded it was decidedly on the wane.

But in spite of specific and of general disturbing factors we obtain consistently positive and, under favorable circumstances, very high coefficients of correlation between tension ranks of the *S*s on different days. Correlations might well have been lowered, not only by errors in measurement, but by a number of factors which doubtless affect tension and which could not be brought under control, *i.e.* state of the *S*'s health, his activity immediately preceding the experiment, experiences of the day, etc. It is to be expected also that adaptation to the repeated presentations of the experimental situation would proceed at a different rate with different *S*s. For these reasons correlation as close as that found between Series I and II or Series IV and V can be accounted for only by assuming (1) that

[17]All coefficients of correlation were obtained by the method of rank differences unless otherwise stated.

[18]The used hand is made the basis for comparison since, in this experiment, it proved to be a better measure of tension than the unused hand.

individuals tend to preserve their ranks with respect to tension in a given situation, and (2) that the technique here employed for measuring tension is reliable.

(*b*) *Constancy during the performance of different tasks.* It is of interest to inquire not only the extent to which individuals preserve their ranks in tension in the *same* situation, but also the extent to which these ranks are preserved in *different* situations. The answer to this question would afford information concerning the extent to which tension is *specific*, or a function of the given situation, and the extent to which it is general, or a characteristic of the individual.

In this investigation, it will be recalled, the *S*s performed two different tasks, (1) making discriminative reactions to pictures, and (2) tapping (under instructions emphasizing speed). Many elements in the two situations were identical, but the differences were sufficient to yield relevant information.

The coefficients of correlation for tension in the corresponding hand during discriminative reactions and tapping were: for the unused hand, 0.15 ± .18; for the used hand, 0.50 ± .14; and for the average of both, 0.58 ± .12.

It is apparent, from the results of the used hand and the average of both, that individuals tend to preserve their respective ranks in tension (though not the absolute value of the tension scores) during the performance of different tasks. It seems safe to conclude, therefore, that the tension manifested by an individual is not entirely specific, or a function of the particular situation, but that it tends to be general, or to characterize the individual in different situations. To what extent situations could be varied without destroying correlations between ranks in tension there are no data for an answer. Our data do indicate, however, that a considerable difference in the tasks assigned does not produce very great variations in the tension ranks of our *S*s. It seems not unlikely that relative degree of tension is a fundamental characteristic of the individual, varying, as all characteristics do, with changes in circumstances, but varying around a central tendency which is different for different individuals.

(*2*) *Significance of individual differences*: (*a*) *Tension and dynamometer scores.* In attempting to determine the significance of individual differences in muscular tension the suggestion occurs that these differences may be due to differences in muscular strength, the stronger individual tending to exert greater pressure upon an object held in the hand. To test this hypothesis the *S*s' strength of

grip was measured by means of a dynamometer. Every effort was made to secure maximum pressure. Three trials were made with each hand, and the final dynamometer score was computed as an average of the highest score from each of the hands. Correlation of these scores with average tension in both hands during discriminative reactions yielded a coefficient of −0.03. Consequently, differences in muscular strength cannot account for differences in muscular tension.

(*b*) *Effect of habituation.* The significance of individual differences in tension of the muscles is disclosed in part through an examination of the effect of habituation upon the tension scores. There are two ways in which this effect may be studied: (1) we may compare tension scores during an early and a later part of a day's performance; and (2) we may compare tension scores during the early and later series of the experiment.

Table III gives tension scores in the unused hand[19] during the first and the last 32 sec. of every series of the experiment. The interval separating these periods was 1 min., 36 sec. The sum of the tension scores of every *S* was obtained for the first and for the last period, and from these scores we obtained the median for the group for each of the periods.

TABLE III

TENSION IN THE UNUSED HAND DURING THE FIRST AND THE LAST THIRTY-TWO SECONDS OF THE DAY'S PERFORMANCE

Series	Median of sums		No. *Ss* showing			No. automobiles pictured*	
	for first 32 sec. (mm.)	for last 32 sec. (mm.)	decrease	increase	no change	first 32 sec.	last 32 sec.
I	160	125	9	3	4	4	3
II	100	90	8	3	5	4	3
III	150	85	9	3	4	2	3
IV	160	95	13	2	1	3	3
V	160	80	13	1	2	3	2

*The appearance of an automobile required a response, and therefore might have affected tension.

[19]The unused hand was made the basis for comparison because it was assumed that tension in this hand would vary less directly with the number of times *S* closed the reaction-key during a given period.

Median tension scores show a decrease from an early to a later part of a day's record, and this decrease appears, not only on the average, but in well over half of the individual cases. It seems probable that, in spite of the short interval separating the two periods compared (1 min., 36 sec.), some adjustment to the situation has been effected by *S*, which is reflected in reduced tension.

The effect of habituation may be observed also through a comparison of tension scores during early and later series of the experiment. But in this comparison we meet again the complicating

TABLE IV

SHOWING TENSION SCORES DURING EVERY SERIES AND FACTORS WHICH MAY HAVE AFFECTED TENSION

Series	Median tension (mm. of mercury)		Av. no. days between records	No. *Ss* absent during week before record taken	No. *Ss* showing decrease in tension from previous record	
	unused hand	used hand			unused hand	used hand
I	16.8	21.7		3		
II	13.6	17.9	.9	4	11	13
III	17.1	23.5	15	8	3	3
IV	17.5	22.0	14.5	10	9	9
V	17.4	25.0	4.5	8	9	6

effects of the epidemic of whooping cough and the changes in the experimental procedure made after the recording of Series II. Again we are led to conclude that Series I and II offer the most valid basis for comparison. Table IV gives median tension scores in the used and the unused hand during each series of the experiment, together with certain facts which may be relevant in interpreting these scores.

Series II shows a marked decrease in tension scores in both the median and the individual results. Series III, the point at which changes were made in the apparatus and procedure, shows an increase in tension, in the median and with almost every individual *S*. Here is evidence that habituation decreases tension, and that the introduction of new elements into a situation increases tension; but the evidence for the latter statement is not unequivocal, since the increase noted in Series III may be due to the effects of whooping cough. Series IV and V fail to show the decrease which under ordinary circumstances would be expected.

However, we have unequivocal evidence of a decrease in tension during the latter part of a day's performance and during the latter of the two series recorded before the outbreak of whooping cough. This decrease in tension is due, apparently, to habituation, which involves in all probability greater ease of adjustment, less useless expenditure of energy, less anxious expectancy.

(*c*) *Tension scores and pressure on the reaction-key.* Further data on the significance of individual differences in muscular tension may be obtained through an examination of the relation between tension scores and degree of pressure upon the reaction-key when making responses to the pictures.

It will be recalled that the reaction-key was constructed to yield a measure of differences in degree of pressure exerted upon it. Pressure scores were obtained for Series III, IV, and V for 15 *Ss*.[20] The rank of the *Ss* in total pressure score (the sum of the scores in the 3 series) was correlated (1) with the average tension in both hands during all series of the experiment, and (2) with the average tension in the *used* hand during Series III, IV, and V. Table V presents the results.

TABLE V

COEFFICIENTS OF CORRELATION BETWEEN TENSION SCORES AND PRESSURE ON THE REACTION-KEY

Both Hands (All series)	Used Hand (Series III, IV, and V)
r .63 P.E. .11	r .24 P.E. .17

Pressure scores are closely related to average tension in both hands during all series of the experiment (r = 0.63), and, surprisingly enough, are only slightly related to tension in the used hand (the hand exerting pressure upon the key) during the particular series for which this pressure was measured. Two interpretations may be suggested. It seems possible that pressure upon the key was an alternative form of expression of tension, so that tense *Ss* who pressed the key with great force exerted correspondingly less pressure upon the rubber grip in the used hand, thus reducing the correlation between what might be conceived of as two separate measures of tension. It seems possible, on the other hand, that

[20]One *S* in the discriminative experiments often touched the reaction-key too lightly to close it, because he disliked the sound of the buzzer. This form of response persisted after the buzzer had been removed. For this reason his pressure scores were discarded. For an explanation of the omission of pressure scores for Series I and II see footnote 15, p. 148.

pressure upon the key, or *force* of reaction, is a habit of response closely connected with habitual tendencies with respect to tension of the muscles, and that these habitual tendencies are indicated more adequately by tension scores from both hands during all series of the experiment than by the more limited sampling of tension represented by scores from the used hand in Series III, IV, and V. The suggested relation between muscular tension and force of reaction finds abundant confirmation in ordinary observation. Tense individuals may be observed to walk with a heavier tread, speak with greater intensity, exert greater pressure in writing and, in general, give evidence of greater energy of response. Occasionally these effects are masked by inhibition; but they are usually observable in greater or less degree. These general observations must be distinguished from the experimental data, which indicate only that high tension scores (when the measures are taken from both hands on 5 separate occasions) correlate rather closely with heavy pressure on the reaction-key.

(*d*) *Tension scores and ratings.* Three nursery school teachers in daily contact with the *S*s were asked to rank them in excitability. The instructions were as follows: "Rank the children from highest to lowest in excitability, *i.e.* tendency to become excited. This trait should not necessarily be identified with 'emotionality' or 'instability.' Excitable individuals are often described as 'keyed-up,' 'high-strung,' 'tense,' or 'stirred-up'." After the rating had been completed the teachers were asked to describe as fully as possible the traits they had had in mind in their classification of the *S*s. Traits mentioned were the following: high, shrill voice, screaming, exuberance, nervous tension, intense and frequent manifestations of emotion, type of reaction when asked for any reason to leave the group. The teachers had previously been asked by another experimenter to rate the same group of *S*s on stability under usual and unusual circumstances. When questioned as to whether their ratings on excitability were different from the ratings they would have given on stability, they all replied that they were not. We must conclude, then, that the ratings probably represent a composite of traits variously termed excitability, stability, adjustment to routine, etc., and we should not attempt to interpret them more specifically.

On the basis of adjustment to the nursery school, the physician and the nurse at the Institute separated the *S*s into 3 categories: poor, average, and good.

TABLE VI

CORRELATION OF AVERAGE TENSION SCORES WITH RATINGS ON EXCITABILITY AND SCHOOL ADJUSTMENT

	Ratings	
	Excitability	School Adjustment†
Unused Hand	.29 P.E. .16	.58
Used Hand	.58 P.E. .12	.61
Av. Both*	.52 (.60) P.E. .13 (.11)	.58

*The figures in parentheses represent the correlation with ratings done at *E*'s request. All other figures represent correlations with composite ratings, *i.e.* ratings which include the earlier ratings on stability.

†The maximum coefficient of mean square contingency which can be obtained on the basis of 3 categories is 0.816.

Table VI gives, for tension in the muscles of the hands, coefficients of correlation with the ratings of the teacher, and coefficients of mean square contingency with the ratings of the physician and nurse.

With the exception of the correlation between tension in the unused hand and ratings on excitability,[21] our coefficients of correlation and our coefficients of contingency are uniformly high or moderately high. We have here evidence of a striking tendency for high tension scores to be associated with traits defined as excitability, instability, etc., and with a type of adjustment to the nursery school which is rated 'poor.'

These correlations afford further evidence that the relative degree of tension of the muscles of an individual is not fortuitous, but is highly significant. They indicate definitely the direction in which we are to look for this significance, *i.e* in the individual's degree of adjustment to his environment, his stability, his degree of susceptibility to excitation.

(3) *Relative merit of various measures of muscular tension.* Three different measures of muscular tension have been referred to in the course of the discussion, (1) tension in the used hand, (2) tension in the unused hand, and (3) tension in both hands, or the average of the first two measures. To what extent do these measures correlate with each other, and what are their relative merits as indicators of tension? Table VII gives intercorrelations of the various tension measures.

There is very little correspondence between tension in the used and the unused hand during discriminative reactions. We have a low positive correlation which is statistically unreliable. Tension in the used and tension in the unused hand during tapping yield a significant positive correlation (r = 0.58). The unused hand during discriminative reactions shows a much lower correlation with the average of both

[21]For an explanation of the low correlation found here see pp. 156-158.

TABLE VII

COEFFICIENTS OF CORRELATION BETWEEN VARIOUS MEASURES OF TENSION

Hand	Test		
	Discrimination		Tapping
	Used Hand	Av. of Both	Used Hand
Unused	.13 P.E. .17	.46 P.E. .14	.58 P.E. .11
Used		.92 P.E. .01	

than does the used hand. But this may be explained by the fact that the scores for the used hand were numerically larger and therefore exerted more influence upon the average.

In regard to the relative merit of the various measures of tension we have two scores of information: (1) a comparison of their coefficients of correlation with the ratings, and (2) a comparison of their coefficients of correlation with tension in the corresponding hand (or hands) during tapping.

Table VI gives coefficients of correlation and coefficients of contingency between ratings and various measures of tension. The tension measures which correlate most closely with the ratings are those from the used hand and those which represent an average from both hands (influenced chiefly by the numerically larger scores from the used hand). The unused hand makes a poorer showing, though the coefficient of contingency between the scores from this hand and the ratings on school adjustment is surprisingly large in the light of facts to be presented later.

A comparison of the various coefficients of correlation between tension in corresponding hands during discriminative reactions and tapping indicates that scores from the used hand during these two performances correspond much more closely than do scores from the unused hand. Scores representing the average from both hands correspond more closely than the scores from either hand taken alone. The coefficients of correlation are: unused hand, 0.15 ± 0.18; used hand, 0.50 ± 0.14; average of both, 0.58 ± 0.12.

From this evidence it appears that the scores from the used hand are, in this experiment, a more adequate measure of tension than the scores from the unused hand. It seems possible, though here the data are not clear, that the average of both these scores (which is determined to a greater extent by scores from the used than by scores from the unused hand) is a better measure than scores from either hand taken alone.

But it is important to note that the relative inadequacy of tension scores from the unused hand is more likely to be a result of particular conditions of the experiment than of any fundamental difference between the two hands as a source of tension measurement. The explanation which seems most plausible is that the rubber grip of the dynamograph held in the unused hand was not sufficiently sensitive to differentiate individuals adequately with respect to tension in that hand. It was made of rubber of the same thickness as that of the dynamograph for the used hand, but since pressures from the used hand were greater, the lack of sensitivity of the dynamograph would not operate so disadvantageously in that case. This explanation is supported by the fact that in tapping, where the pressures were uniformly greater, the scores from the used and the unused hand cor-

relate considerably more closely (r = 0.58) than in the discriminative reactions (r = 0.13). That the small range of scores is a possible explanation of the relative inadequacy of tension measures from the unused hand during the discriminative reactions is indicated by Table I. In an earlier experiment in which very thin rubber was used for the dynamograph in the unused hand, tension scores from this hand correlated with ratings to about the same extent as scores from the used hand in the present experiment.[22]

It seems reasonable to conclude that the correlation between tension scores in the used and the unused hand during tapping (r = 0.58) is more representative of the actual relation between tension in the two hands than the lower correlation during discriminative reactions. We have no adequate basis for a conclusion as to whether it is of more value in general to measure tension in a used or a corresponding unused part of the body. It seems probable that a combination of these two measures would be of greatest value, first, because correlations in this experiment were occasionally raised by combining the two measures and, second, because it seems reasonable to suppose that individuals would differ in the degree to which they shunt tension into a used or a corresponding unused part of the body. If this is true, the most adequate measure of tension would include a sampling from both an active and a quiescent region.

Summary

Our *S*s differed markedly with respect to tension in the muscles of the hands in the same experimental situation. During successive performances of the same task (discriminative reactions) and during the performance of a different task (tapping), they tended to maintain they respective ranks in tension. Apparently tension differences are relatively constant and are not entirely the function of a specific situation.

In attempting to demonstrate the significance of individual differences in muscular tension, we have shown (1) that these differences cannot be explained by differences in muscular strength, (2) that habituation to a situation tends to decrease tension, (3) that individuals with higher tension scores tended to press the reaction-key with greater force, and (4) that individuals with higher tension scores tended to be rated lower on stability and degree of adjustment to the environment.

On the basis of these data it appears that we have in this technique of the measurement of muscular tensions a very promising approach to a study of individual differences in emotional tendencies.

Discussion and Conclusions

It seems entirely consistent with psychological fact and theory to conclude that individual differences in tension of the skeletal

[22] Duffy, *op. cit*, 33.

muscles are associated with differences in emotional tendencies.[23] Increased tension has been observed by many investigators under a variety of circumstances, *e.g.* sensory stimulation, problem solving, the introduction of distractors, and, less often, emotional stimulation.[24] From the evidence at hand it appears possible that increases in tension signify an increase either in excitation in general or in that particular type of excitation called 'effort.' They appear to be associated with striving and expectancy and other states in which the organism is exerting or preparing to exert increased energy. Without question, excited emotional states are among those in which such increases in muscular tension are to be expected; for they are often described as aroused, watchful states, states of increased effort, by which the organism is attempting to meet an emergency. But, as we have pointed out, tension increases have been found not only in excited emotional states, but in other aroused or effortful states, including states of mental work. Why, then, should such increases in tension be considered useful in a study of the emotions?

To answer this question we must review briefly our present knowledge of the emotions. Physiological studies of the emotions have shown that certain bodily changes occur during emotional states, but these studies have failed (1) to discover patterns characteristic of particular emotions, and (2) to reveal in any clear-cut way characteristic differences between aroused emotional states and states of mental work. The changes in pulse rate, respiration, galvanic phenomena, etc., found in emotional states have been found also during intense mental effort. It is obvious that active mental and excited emotional states possess a large group of common elements. These common elements, we would maintain, constitute the excited or aroused quality of the states in question; they produce the generally observed dynamogenic effects. It seems probable that our various physiological measures or indices have been, in actuality, indices of the extent to which the organism was *aroused*, the extent of the process of dynamogenesis, and not indices *per se* of an emotional as opposed to a mental state.

[23] The reference throughout the article is to tensions of the muscles which are assumed to be 'involuntary.'

[24] For a discussion of the effect of sensory stimulation upon the skeletal muscles see Ch. Féré, *Sensation et Mouvement*, 2 éd. rev., 1900, 1-170. Investigations of the effect of emotional stimulation are mentioned in my introduction. For the effect of mental work and certain other conditions see A. G. Bills, Mental work, *Psychol. Bull.*, 28, 1931, 505-529.

But, in practice, these physiological measures have been useful as indices of states called 'emotional.' The reason for this is to be found in the current definition of an emotion. Psychologists have been unable to draw an objective distinction between mental states and emotional states except on two arbitrary bases: (1) degree of disturbance in the organism, and (2) knowledge of the stimulus. Marked degrees of disturbance in the organism, if they are not due to obvious 'physical' causes, are attributed to the presence of 'emotional stimuli,' and the state of the organism is called an 'emotional state.' Thus a student taking an examination would be called *mentally* alert or aroused if his pulse rate showed a slight increase, but he would be called *emotionally* aroused if the increase were very great. Degree of excitation has been the criterion for such determinations. But how great a degree of excitation is necessary to determine a difference in kind, *i.e.* to delimit an emotional from a mental state, has not been established. Consequently, it has been necessary to employ a second standard to meet the deficiencies of the first. To this end psychologists have engaged in a search for the stimulus which gave rise to a particular state. But very often in practice 'kind of stimulus' is decided from 'degree of excitation'—*e.g.* the 'emotional' quality of a stimulus-word is determined from its effect on pulse rate or blood pressure.

Degree of muscular tension affords a very obvious but largely neglected index of degree of excitation and consequent dynamogenic effect. Differences between individuals in tension of the skeletal muscles in the same situation appear to indicate differences in the extent to which the situation has 'excited' or 'aroused' the respective individuals; and habitual differences between individuals in tension of the muscles appear to indicate differences in their general tendency to become 'aroused' or 'excited,' and thus to indicate differences in temperament. These differences in 'dynamogenic tendency' are of great importance and deserve study apart from their rôle in so-called 'mental' or 'emotional' states. Increase in dynamogenic effects within the organism, whenever found, is associated with marked changes in behavior. Among these changes are, in all probability, increased speed and force of reaction, and greater impulsion, or increased difficulty of inhibition. If such facts are established, we may in the future be able to describe certain patterns of reaction which occur in excited states and which, therefore, tend to characterize individuals who become frequently and easily excited. These

data would have wide applications, since there are, undoubtedly, certain tasks or vocations for which such individuals would be particularly fitted and, conversely, certain tasks at which they would be handicapped. Great promise is held in this type of approach to the study of temperamental tendencies. It seems possible that it will substitute, for arbitrary classifications of 'key' traits of personality, experimentally established physiological bases for temperamental differences. Such bases would enable us to predict correlations between traits because of the derivation of these traits from a common source. But this does not imply that such a group of traits would not be subject to variation, since changes in general physiological condition or in environment may at any time produce changes in characteristics.

To return to a consideration of dynamogenic tendency, it is unsafe to assume that a given degree of excitation will result always (in either the same individual or in different individuals) in precisely the same effect upon behavior. To illustrate, we may consider the relation between degree of muscular tension (an index of excitation) and efficiency of performance in mental tasks. Several investigators have reported that increased efficiency accompanies certain increases in tension. Our data,[25] to be reported in a subsequent article, indicate that, for our group of subjects as a whole, there was a moderately close relationship between high tension and poor performance. But there were instances, both here and in our earlier study, in which individuals with a relatively high tension score gave an excellent performance. The explanation for this, as suggested in the earlier publication and supported by subsequent data, is that excellence of performance depends not alone upon degree of tension but also upon ability to coördinate reactions. Coördination becomes more difficult under high degrees of tension, and hence we find in the majority of cases (at least with young children) that high tension decreases efficiency. But, when an individual attains good coördination under high tension, his performance may surpass that of his own normal level as well as that of other individuals with a lower degree of tension. We further suggested, and this seems increasingly probable, that differences in ability to coördinate reac-

[25]These data, together with the interpretation outlined below (which suggests an explanation for the lack of consistency of results in this field of experimentation), were first presented by the writer to the New York Branch American Psychological Association, April 18, 1931.

tions might be revealed by the type of tension tracing with respect to regularity or smoothness of line. Our best performer in the earlier experiment was a subject with a relatively high but smooth tension line. This evidence illustrates the point that, in discovering an individual's tendencies with respect to degree of muscular tension, we obtain extremely valuable information, but not information which may be interpreted without other considerations when we attempt to use it for a prognosis of behavior.

We return now to the original question—the value of the measurement of muscular tensions as a technique for the study of emotional tendencies. It is obvious that increases in tension of the skeletal muscles have a wider occurrence than in states called 'emotional.' But until we formulate a more precise definition of an emotion, degree of muscular tension will constitute an excellent and easily available index of excited 'emotional' states. In common parlance we call those individuals 'excitable' who respond with intensity (or show strong dynamogenic effects) to 'emotional' stimuli which cause little or no response in other individuals. If such reactions occur with relatively great frequency and intensity we call the individuals manifesting them 'unstable' or 'poorly adjusted.' It is not surprising that we found a rather high correlation between average degree of tension of the muscles and ratings on excitability, stability, and degree of adjustment to the nursery school.

Although it may not be possible to offer a final solution for the theoretical problems suggested here, it is practically useful to determine which individuals manifest frequently a highly aroused state. A tendency of this sort might indicate an environment that was uncongenial or threatening, or it might indicate firmly established habits of reaction in the individual, modified only slightly by specific environmental circumstances. Habitually high degrees of tension of the muscles serve to reveal individuals who are living under conditions of strain, effort, or 'over-reactivity.'

RELATIVE EFFECTS OF DETERMINATE AND INDETERMINATE *AUFGABEN*

By D. W. CHAPMAN, Harvard University

The psychologist who recognizes the obligation of describing experience in systematic terms has always, sooner or later, had to embark upon the analysis of certain typical phenomenal fields which he considered to be representative of experience at large. This necessity is as obvious and compelling to the configurationalist as it ever was to the structuralist; but with the passage of time, and with the accumulation of experimental evidence, we can point more confidently to errors which inhere in certain methods of analysis. Indeed, we may consider these errors themselves as proper objects of scientific interest, inasmuch as around them must operate the modern methodological reorientation.

If a given conscious complex was to be analyzed, the older psychology proceeded confidently by coercing its part-contents, a, b, c, d, e, successively into the focus of attention, and by there describing them under favorable conditions of clearness. In symbolic terms, this operation consisted in the establishment (under constant stimulus-conditions) of consciousnesses representable as *Abcde*, *aBcde*, *abCde*, etc., wherein a capital letter indicates the attentional focality of a part-content. The focal terms were then collected in the form $A+B+C+D+E$, and this aggregate was taken to represent the complete analysis of *abcde*.

But this operation is justifiable only if we assume that A and a, B and b, C and c, etc., are identical. Certainly we know that in respect of clearness they are not identical; for it is precisely by a change in clearness-value that they enter or leave the focus of attention. Moreover, we are acquainted with common instances (the analysis of clangs and flavors) where such a shift of attention appears to invite the emergence of actually new qualities, by no means positively identifiable with the just previous experience. Since in the experimental situation such shifts of attention are the effects of

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changes in the *Aufgabe* with which the observer approaches the stimulus-presentation, we may state the problem met by the analytical approach to be the following: *Instead of finding, for a constant stimulus-presentation, one experience over which attention can 'sweep' analytically, we must suppose that for this constant presentation there are* n *possible phenomenal fields, each with its particular attentional structure, and each determined primarily by one of* n *corresponding* Aufgaben. *There is hereby given the possibility that reportable aspects of one field may occur only in vaguely reportable form, or not at all, in another field.*

Of the experimental investigations which have dealt with the demonstration and measurement of this effect, two especially deserve recognition.

The first is that of Külpe[1]—the abstraction experiment—wherein he presented tachistoscopically to his *Os* colored, printed syllables, about which could be reported the number of letters involved, the colors involved, the positions of the colors, or the nature of the total figure which they composed. In some presentations the *Aufgabe* was made indeterminate, and, only after the *O* had reported such characteristics as he chose, was he required to report on the remaining ones; in other presentations the *O* was predetermined for certain characteristics, reported on them, and then subsequently was asked about the others. Külpe found a universally higher reportability for aspects of the stimulus favored by the *Aufgabe* (*i.e.* the preëxposure *Aufgabe*) than for aspects not so favored; and he announced the general law that "the most numerous, most correct, and most definite reports occur in those cases where report and *Aufgabe* correspond [wo die Aussagen mit den Aufgaben zusammenfallen]."[2]

The second study is that of Yokoyama, reported by Boring.[3] Yokoyama presented his *Os* tachistoscopically with pairs of rectangles of light, which he varied in intensity and length; and the *O* was required to make comparative judgments with respect to these attributes. "In one series the *O* was predetermined to observe relative intensity. He was to pay attention to intensity only and to judge the one stimulus with respect to the other. The instruction was vigorous, the sensory difference was small, and the task was continued for many observations. Altogether the *O* may be supposed to have been as definitely predisposed for intensity as he could have been. At irregular intervals in this series, however, and not oftener than once in fifty trials, nor more than twice in a session, the *E* broke in, immediately after the exposure and often before the judgment of intensity could be voiced, with a secondary instruction: 'How about extent?' The *O* was then expected to give, *ex post facto*, a judgment of relative extents. In other series the primary determination was for extent, and intensity was called for at infrequent intervals." The conclusion reached, with respect to the phenomenon of diminution of reportability, was that "it is plain that the

[1]O. Külpe, Versuche über Abstraktion, *Ber. ü. d. I. Kongr. f. exper. Psychol.*, 1904, 56-68.

[2]*Ibid.*, 11.

[3]E. G. Boring, Attribute and sensation, this JOURNAL, 35, 1924, 301-304.

original perception under the primary determinant is inadequate to a judgment of any difference other than the attribute upon which the primary determinant is directed."[4]

These two studies establish the general principle that the phenomenal field initiated by a stimulus-presentation does vary in the adequacy of its several aspects, and that this variation depends upon the predetermination of the *O*. Neither of them, however, appears capable, in its technique, of measuring the phenomenon in a satisfactory way.

In Külpe's experiment, we should observe that the secondary, "surprise" *Aufgabe* was most often given *O* only after he had had a sufficient length of time for a report on the primary *Aufgabe*. This means that if we call for a report upon aspect A in a given phenomenal field, and later call for a report on aspect B of the same field, and our results show that reports on B are less accurate than those on A, we shall not know which of the following cases is true: (1) that B was less distinct than A in the original perceptual field, because B was not predisposed-for; (2) that B was less adequately reported because, between the times of report on A and report on B, forgetting with respect to B had taken place; and (3) that B was less adequately reported because the intervening act of reporting A tended to inhibit or modify surrogates of B.

Külpe took case 1 to be the fact. Rubin,[5] however, has argued case 2; and Wilcocks,[6] following this suggestion, has shown by an experiment, close to Külpe's in detail, that where a question is asked corresponding to the predetermination, but some time after presentation, the correctness of report on this question may be lower than for a question asked more quickly after presentation and not favored by the predetermination. The phenomenon of rapid forgetting appears therefore to cut across the phenomenon which Külpe hoped alone to demonstrate.

Yokoyama's experiment probably avoided this error to a greater extent, for the surprise-*Aufgabe* was given often before the report in respect of the primary *Aufgabe* could be made. But Yokoyama's technique, since it requires such infrequency of secondary *Aufgaben*

[4] *Ibid.*, 303.

[5] E. Rubin, Bericht über experimentelle Untersuchungen der Abstraktion, *Zsch. f. Psychol.*, 63, 1913, 386.

[6] R. W. Wilcocks, An examination of Külpe's experiments on abstraction, this JOURNAL, 36, 1925, 324-340.

as genuinely to surprise *O*, has the disadvantage of affording only two cases in an experimental hour. It does not, in brief, lend itself to quantitative treatment.

The present experiment sought to reduce these difficulties in the following way.[7] We can present to *O* stimulus-patterns (arrangements of letters) about which a number of types of report (in this case three: number, names, location) can be made. We can, in one series of presentations, give a definite *Aufgabe* before each exposure, and obtain reports under that condition. We can then present a second series to *O* in which the definite *Aufgabe* is given him only very quickly after each exposure. His *Aufgabe* in this case (at least in the long run) can be considered as relatively indeterminate at the time of stimulus-exposure. We shall expect, if we score for both series percentages of material correctly reported against numbers of stimuli simultaneously presented, that the results will approximate the $\Phi(\gamma)$ relation, as Fernberger has demonstrated;[8] and we shall be able to obtain a 'cognitive' limen[9] for each of the *Aufgaben* under each of the two conditions of determinate and indeterminate instruction. Comparision of the two sets of limens will then furnish us with a measure of the loss in reportability which the latter type of instruction effects.

This procedure offers the following advantages over the procedures of Külpe and Yokoyama: (1) The interval of time between the exposure and the giving of the surprise-*Aufgabe* can be kept reasonably short and constant.

(2) In no case will the necessity arise of making one report before responding to this surprise-*Aufgabe*.

(3) A sufficient number of cases of report with surprise-*Aufgabe* can be obtained to make their statistical properties comparable with those obtained from *Aufgaben* preceding the exposure.

(4) The factor of surprise involved in the *Aufgabe*-after-exposure will tend to remain constant, since *O* will act, practically, with knowledge of the experiment, *i.e.*, that one of three tasks is to be required of him.

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[7]The writer wishes to acknowledge his indebtedness to Professor C. C. Pratt, under whose guidance this study was performed.

[8]S. W. Fernberger, Range of visual apprehension, this JOURNAL, 32, 1921, 121-133.

[9]A. D. Glanville and K. M. Dallenbach, Range of attention, *ibid.*, 41, 1929, 207-236.

The experimenter realizes, of course, that in adopting this technique, he sacrificed the possibility of determining what happens to the reportability of characteristics when *O* is set maximally to observe other characteristics. He believes, however, that his procedure enabled him to investigate in a far more reliable way the same effect that Külpe pointed to, namely, the difference between reports which are definitely demanded before the exposure of material, and reports which are definitely demanded only *ex post facto*. This experiment deals, then, with reports made under an *Aufgabe* intermediate between definite determination toward a critical aspect and definite determination away from that aspect.

Method

Stimulus-material. The stimulus-cards used in this experiment consisted of squares of white, unglazed drawing paper, 7 in. on a side. A centered rectangle was ruled off on each card in pencil, 3 3/4 in. wide and 3 1/8 in. high; and this rectangle was then ruled into five 3/4-in. columns intersecting five 5/8-in. rows, thus giving 25 small rectangles, 3/4 by 5/8 in. each.

The stimuli—black printed letters, 1/2 in. high, cut from Dennison's gummed alphabet no. 31—were then pasted into various of these rectangles, and the penciled rulings were removed with art gum. The letters were arranged on the cards haphazardly, except for the following rules: (1) all letters used were capitals; (2) in order to prevent the formation of syllables, no vowels were employed, and Y was considered a vowel; (3) M and W, because of their extraordinary breadth in this alphabet, were omitted; (4) the letters were always arranged in a line such that successive members were vertically, horizontally, or diagonally adjacent—this was done in order to minimize subjective sub-grouping; (5) no two cards bore the same arrangement of letters; and (6) all the letters appeared in the course of the experiment with approximately the same frequency.

The preliminary work showed that, for the method of constant stimuli, it would be advisable to work in the range of 4 to 8 stimulus-objects. *E* therefore made 18 cards bearing 4 letters each, 18 cards bearing 5 letters each, and so on, up to and including 8 letters,—a total of 90 cards.

Apparatus. The stimulus-cards were presented in a moderately darkened room by means of a modified Dodge tachistoscope. The tachistoscope was essentially a light-proof box, provided with an observation slit to which *O* held his eyes during exposure. The stimulus-card was held within a dead black frame 18 in. before *O*'s eyes. Between his eyes and the card, a sheet of glass set at 45° to the line of regard reflected a moderately bright white field so as to make it appear coincident with the stimulus-card. This constant field entirely hid the letters on the stimulus-card until the latter was illuminated for an instant (by a beam of light reflected off the glass on the opposite side to the constant field), during which instant the black stimulus-letters stood out on the constant white field. The illuminating mechanism was a rotating-disk episcotister, revolving in front of a projection lantern, and fitted with a variable slit to give exposures of

from 10 to 60 σ. Since this episcotister involved a fast and a slow disk, geared together, it gave one flash of light every 1.2 sec., if the projection lantern were continuously illuminated. A single flash could be obtained by turning on the lamp somewhat before the coincidence of the slits, and turning it off immediately after. This was made possible by a timing light which flashed on *E*'s control-desk. The only noise incident to the operation of the apparatus was the gentle humming of the constant-speed motor which drove the episcotister.

The instructions for each exposure were given *O* by means of an instruction-box, the front of which bore three small milk-glass windows, on each of which was printed an *Aufgabe* (count, locate, name). Keys at *E*'s desk enabled him to light a lamp behind the window bearing the instruction for any given exposure. The instruction-box was placed outside the tachistoscope and somewhat to the right of *O*.

A warning signal was furnished by a telegraph sounder.

A narrow board, fixed to the edge of the table in front of the tachistoscope, provided a desk on which *O* could write his report. The desk was illuminated by an overhead light which *E* could switch on immediately after each exposure.

Procedure. The experiment was performed from the beginning of November, 1929, through March, 1930.

The *O*s were 6 in number: Dr. C. L. Goldthwait (*G*), Messrs. W. H. Hunt (*H*), M. H. Lewis (*L*), K. W. Oberlin (*Ob*), and Misses H. Nye (*N*), and J. I. Steinberg (*St*). Excepting *N*, an undergraduate in philosophy with some psychological training, the *O*s were Harvard and Radcliffe students of psychology; the 4 men were graduates.

Each *O* observed for one experimental hour a week. For *G*, *H*, and *L*, the tachistoscope was adjusted to give a length of exposure of 10σ. *St*, however, had difficulty with this length of exposure, and after the practice-series, was given exposures of 30σ, as were also *N* and *Ob*.

During each experimental hour, each *O* was given 90 exposures of the stimulus-cards, arranged in haphazard order with respect to the number of stimuli which each card bore. Forty-five of these exposures formed a 'P-series,' in which the *Aufgaben* were given before each exposure; and the remaining 45 exposures formed an 'F-series,' in which the *Aufgaben* were given immediately following each exposure. To minimize practice-effects, the P-series was given before the F-series in one experimental hour, and after the F-series in the succeeding experimental hour; and this alternation of order was maintained throughout the experiment.

Instructions. Before he observed the P-series, *O* was given the following instructions to read:

In this experiment you will be presented with a tachistoscopic visual field containing a number of letters of the alphabet. Before each exposure the instruction-box will indicate one of the following three types of report, which you are to give after the exposure:

"Count." In this case you are to report the number of letters seen, by writing that number on the report slip; for example, 7.

"Locate." In this case you are to reproduce with small circles (each circle representing a letter) the relative position of all letters seen; for example,

o o
o o o o o
o

"Name." In this case you are to print out in a horizontal line all the letters seen, irrespective of their positions: for example, F X K C L

After you have determined the instruction from the box, place your eyes to the eye-piece and fixate the bright, square field near its center. The exposure will occur 2 sec. after the telegraph sounder clicks.

For the F-series, *O* was given the following instructions:

In this part of the experiment the procedure will be the same, except that you are to take your instructions from the box immediately *following*, instead of preceding, the exposure."

For each exposure in the P-series, the procedure was as follows, the times at the left indicating flashes of the timing light on *E*'s desk:

Time	*Operation*
	O signifies "ready."
0 sec.	Outside light turned off.
	Instruction-box turned on.
2 sec.	Warning signal given.
	Instruction-box turned off.
4 sec.	Exposure occurs.
	Outside light turned on.
	O writes report.

For the F-series, the procedure was as follows:

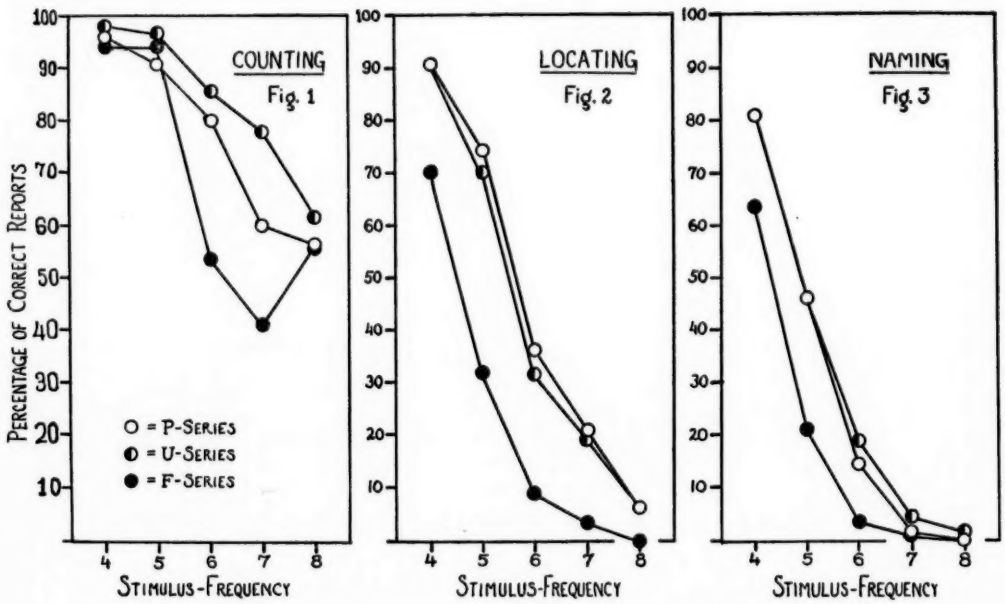
	O signifies "ready."
0 sec.	Outside light turned off.
2 sec.	Warning signal given.
4 sec.	Exposure occurs.
	Instruction-box turned on.
	Outside light turned on.
	O writes report.

In order to determine whether in the P-series the 4-sec. interval between the lighting of the instruction-box and the occurrence of the exposure had been sufficient to allow *O* to establish a firm and definite *Einstellung*, *E* conducted a 'U-series' over 3 more experimental hours. During the first of these hours *O* did nothing but locate the stimuli on 75 cards presented him; in the second hour he did nothing but name, and in the third hour nothing but count. This series was used as a control to determine whether an *Einstellung* maintained over an experimental hour was significantly more firm than one established only shortly before each exposure in the P-series.

Results

The *Os*' reports were scored right or wrong. Table I gives the total number of correct reports made by each *O* (all 5 stimulus-frequencies pooled) for each *Aufgabe*, and arranged according to mode of presentation of *Aufgabe* (columns P, F, and U). Differences between the results of various pairs of series are tabulated in the columns headed D_{F-P}, D_{F-U}, D_{U-P}.

Two generalizations are apparent, and are exhibited in most striking form by the results for locating and naming: (1) the results from the P- and U-series approximate each other; and (2) the number of correct reports in the F-series is distinctly lower than in the P- or U-series. The results from counting are somewhat less univocal, and we shall note the probable reason for this presently.



FIGS. 1–3. SHOWING THE PERCENTAGES OF CORRECT REPORTS FOR EVERY INSTRUCTION AND FOR EVERY EXPERIMENTAL SERIES

TABLE I

TOTAL NUMBER OF CORRECT REPORTS (OUT OF 75) MADE BY EVERY *O* FOR EACH OF THE P-, F-, AND U-SERIES, FOR EACH *AUFGABE*.

	O	P	F	U	D_{F-P}	D_{F-U}	D_{U-P}
Counting	*G*	56	44	50	−12	− 6	− 6
	H	49	47	51	− 2	− 4	2
	L	54	43	63	−11	−20	9
	N	66	59	70	− 7	−11	4
	Ob	61	53	69	− 8	−16	8
	St	60	59	75	− 1	−16	15
	Total	346	305	378	−41	−73	32
Locating	*G*	28	18	32	−10	−14	4
	H	38	18	27	−20	− 9	−11
	L	30	15	25	−15	−10	− 5
	N	32	12	33	−20	−21	1
	Ob	50	24	42	−26	−18	− 8
	St	29	16	38	−13	−22	9
	Total	207	103	197	−104	−94	−10
Naming	*G*	16	12	11	− 4	1	− 5
	H	18	13	17	− 5	− 4	− 1
	L	14	12	17	− 2	− 5	3
	N	25	15	29	−10	−14	4
	Ob	29	18	27	−11	− 9	− 2
	St	28	10	37	−18	−27	9
	Total	130	80	138	−50	−58	8

The data for all 6 *O*s are pooled and presented graphically in Figs. 1-3, in the form of unsmoothed curves. Here the relative inefficiency of report in the F-series is apparent, as is also the approximation of the P- and U-series for the *Aufgaben*, locating and naming.

Since the curves for locating and naming are free from inversion and strongly suggest the ogive function, their limens were calculated from Urban's tables, and found to be as follows:

		L[10]	h
Locating	P-series	5.80	−0.509
	U-series	5.49	−0.506
	F-series	4.54	−0.594
Naming	P-series	4.91	−0.684
	U-series	4.98	−0.555
	F-series	4.28	−0.700

[10]L is expressed in stimulus-units.

These limens would seem to offer themselves as acceptable measures of the efficiency of report characteristic of the three series.

The curves obtained from counting are not suitable for a calculation of limens, chiefly because of their tendency to inversion at a stimulus-frequency of seven. An analysis of the reports revealed that the *Os* called 6 stimuli 'seven' more often than 'five.' They soon learned that 8 was the terminal frequency in the range, and this tendency to identify the larger groups of stimuli as 'eight' appears to have resulted in an abnormally large percentage of correct reports on 8 stimuli—an artificiality of the experiment which presumably would not have occurred had the range of frequencies been larger.

We may conclude, in fine, from the quantitative data that the efficiency of report for all tasks is lower under an indefinite *Aufgabe* than under a definite instruction. We may also conclude that the efficiency of report under a definite *Aufgabe*, established 2 sec. before the exposure of the stimulus, is approximately the same as the efficiency of report under a definite *Aufgabe* carried throughout an experimental hour.

Introspective reports taken during the final experimental hour show that the better reportability of the P-series partly resides in perceptual differences in the phenomenal field following definite *Aufgaben*:

St: "When carrying the instructions for locating, I attempted to adopt a set for the total configuration of the field. The whole pattern then became stamped in so that I could still see it while reporting. In naming, the set was for a less organized pattern, so that the parts could be seen as lesser units. In counting, the set was to adopt a loose organization, so that the individual letters easily abstracted themselves."

G: "During the exposure it [the phenomenal field] was the same for locating and counting, but for naming the details of the letters stood our more clearly."

N: "With the shorter groups of stimuli, all three characteristics were clear [number, position, and identity]. As for the larger groupings: in counting, the other characteristics were vague at most; in locating, the number of letters was usually clear; and in naming, I concentrated so hard on the individual letters, that the group as a whole was vague."

L: "For the numerical instruction, it [the phenomenal field] had quite a discrete character; for locating, a definite geometrical character; and for naming, it had a combination of the two, with greater differences in the value of the parts."

H and *Ob* found no differences in the structure of the field for differing preceding *Aufgaben*.

Our evidence therefore permits us to say that, in general, an *Aufgabe* given before the presentation of a field tends to emphasize

in that field perceptual characteristics which will facilitate the report which it demands. Further introspections add the fact that where this accentuation is found during the exposure of the material, it persists in subsequent surrogative processes, and may even increase, until the report has been made. This effect of a previous *Aufgabe* is therefore common both to the immediate phenomenal field and to its surrogates; and for this reason, we cannot speak of efficiency of report as revealing solely differences of structuring in the original phenomenal field, without admitting the possibility that this efficiency is also governed by structural changes which have taken place in the surrogates intervening between the original field and report.

In the F-series, on the other hand, the typical field and its surrogates have a less univocal structure, corresponding to the neutral *Aufgabe* with which the presentation is approached. When the subsequent definite *Aufgabe* impinges upon this surrogative mass, there is an attempt on the part of *O* to restructure the surrogate along dimensions which are essential for report—and it is apparently the difficulty of this process which is reflected in the lesser accuracy of report in the F-series:

G: "I think the difference [in accuracy of report] was due to the necessity of carrying the mass of unnecessary imagery [in the F-series]. It seemed to be beyond my capacity to hold so much with a clearness anything like that possible when the instruction was given previous to exposure."

St: "In making the report [in the F-series] it was often necessary to change the configuration of the field. Difficulties resulted, in which there was a confusion of the three characteristics with location losing the most strength, and naming and counting following."

N: "I should say that it [the lesser accuracy] was due simply to the confusion resulting from counting, locating, and naming all at the same time."

G: "Considerable tension from moment of exposure until instructions perceived. Confused mass of visual, auditory and voci-kinaesthetic imagery held in suspension. When instructions perceived, a pushing out of unnecessary material and attempt to strengthen wanted material. (I do not think the latter ever became clearer, but there was always the effort to cause it to.)"

The picture which these considerations allow us to draw of the relative functions of *Aufgabe*-before-exposure and *Aufgabe*-after-exposure, may be put as follows: In the P-series, the *Aufgabe* initiates a process whereby perceptual characteristics of the stimulus are emphasized according as they are demanded by the *Aufgabe*. The *Aufgabe* may thus be thought of, figuratively, as providing a preliminary matrix to the structural values of which succeeding experience tends to conform. Its first effect is felt in the perceptual field incident

to the exposure of the stimulus. This process of emphasis proceeds during the successive surrogative experience; and the fading out of irrelevant aspects of these images represents an enhancement of the same effect in the original phenomenal field. At whatever temporal stadium in this process the giving of the report begins, it is conditioned by a surrogative mass which shows as its principal aspects precisely those which contribute to the accuracy of report.

In contrast to this, for the F-series, the immediate perceptual field, being preceded by no exclusive *Aufgabe*, tends to exhibit, more or less equally, all the aspects which might be demanded by any of the *Aufgaben* which the *O* knows he may receive. The clearness of these coexistent aspects is diminished by their rivalry. This condition persists in the subsequent surrogates. When the *Aufgabe* is received, there ensues a tendency for this surrogative mass to restructure itself according to the matrix given; but this restructuring is relatively incomplete, and brings with it a decreased liveliness of the surrogate as a whole. The report is then, in this case, conditioned by a less univocal surrogative structure, and loses thereby in accuracy.

Conclusions

In comparing reports in counting, locating, and naming groups of 4 to 8 stimuli presented tachistoscopically in each of the following two series, P-series, wherein the *Aufgabe* was given before presentation of the stimuli; and F-series, wherein the *Aufgabe* was given immediately after presentation of the stimuli, the experimenter finds:

(1) A lower accuracy of report for the F-series than for the P-series;

(2) That the comparison of cognitive limens gives a convenient and practicable index of this difference of accuracy of report;

(3) That this difference is apparently due to (a) the tendency of both perceptual field and succeeding surrogates in the P-series to accentuate aspects demanded by the *Aufgabe*, and (b) the tendency of both perceptual field and succeeding surrogates in the F-series toward an indifference or confusion of aspects, and a resistance to the accentuation demanded by the *Aufgabe;*

(4) That, since the coöperation of both perceptual field and succeeding surrogates is indicated, (a) the hypothesis that the *Aufgabe* in part determines reportable aspects of phenomenal fields is supported, and (b) the hypothesis that efficiency of report measures exclusively the structure of the original perceptual field, apart from its surrogates, is not sustained.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXX. Position *vs.* Intensity as a Determinant of the Attention of Left-Handed Observers

By Annie Mell White and Karl M. Dallenbach

In the various studies on the determinants of attention published from this laboratory,[1] spatial position was found to be an important factor. The position to the left of the point of visual fixation had, for all the *Os* in all of the experiments, a marked attentional advantage. Since all the *Os* were right-handed, the effectiveness of the left-hand position was explained neurologically as follows.[2] Neural excitations aroused in the right retinas of each eye—the areas stimulated by objects occupying positions to the left of fixation—center in the right hemisphere of the brain where they, because right-handed people are left brained, are comparatively free from interference and inhibition by other concomitant processes. As a consequence they possess on the side of consciousness a high degree of attensity, *i.e.* clearness or vividness. Everything else being equal, objects arousing processes in the right brain would enjoy, therefore, an attentional advantage over all others.

This explanation was given added plausibility when Burke and Dallenbach extended the experiments to left-handed *Os*, for they found that the right-hand position had an attentional advantage for the left-handed.[3] They used, however, only 2 *Os*, and only one of these was, according to Rife's classification of dextrality, of the pure LLL type.[4] Their work, therefore, clearly needs confirmation.

*Accepted for publication June 15, 1930.

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K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 205, 213, 229; Position *vs.* intensity as a determinant of clearness, this Journal, 34, 1923, 282-286.
Doris Dewey and K. M. Dallenbach, Size *vs.* intensity as a determinant of attention, *ibid.*, 35, 1924, 121-125.
C. L. Friedline and K. M. Dallenbach, Distance from point of fixation *vs.* intensity as a determinant of attention, *ibid.*, 41, 1929, 464-468.

[2] Dallenbach, *op. cit.*, this Journal, 285. Cf. also H. A. Cohn and J. W. Papez, A comparative study of the visuosensory or striate area in the two hemispheres of the human brain, *Amer. J. Phys. Anthrop.*, 14, 1930, 413 f.

[3] R. S. Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left-handed observers, this Journal, 35, 1924, 267-269.

[4] J. M. Rife (Types of dextrality, *Psychol. Rev.*, 29, 1922, 474-480) found 3 distinct types of right-handedness, and 3 corresponding types of left-handedness, making 6 forms of dextrality in all. He determined these types from the use of the hands in unimanual activities, such as writing, sewing, throwing, and hammering,

The object of the present study was to test the neurological theory further by determining the positional preference of other left-handed *Os*. As in the earlier experiments, every condition of attention except intensity and position was eliminated, and the effect of position was measured in terms of intensity.

Observers. The *Os* were Miss Ethel Shaw (*Sh*) and Mr. Benedict Newell (*Ne*), seniors in Cornell University, and Mr. C. W. Fox (*Fx*), graduate assistant in psychology.

We were unable to obtain *Os* of the pure LLL type. *Sh* is left-handed in writing, eating, throwing, hammering, and in using the scissors, bat, ax and spade, but she uses a pitchfork and broom right-handed and is right-eyed. She clearly belongs to the LLR type. *Ne* is left-handed in all the unimanual activities except writing and eating which he now does right-handed as a result of insistent training when young. He plays tennis, bats, and wields an ax left-handed, but uses a pitchfork and shovel right-handed. He is right-eyed. He too, we believe, belongs to the LLR type. *Fx* is left-handed in throwing, hammering, in using the scissors, ax, spade, shovel, pitchfork, and broom. He is left-footed in kicking a football and in jumping, and is left-eyed. He is, however, right-handed in writing, eating, batting, and playing golf. He clearly does not fall within any of Rife's classes. His formula seems to be $\frac{R}{L}$RL. He was experienced in psychological observation; the other two *Os* were naïve. All of them, however, served in the experiment without knowledge of the specific problem.

Apparatus. The apparatus was similar to that used in the earlier studies.[5] *O* sat in a light-proof booth with his head held firmly in position by means of a biting-board which was so adjusted that the fixation-point, 1 m. away and 1 mm. in diam., would fall, if projected, midway between his two eyes.

The comparison areas, two circular openings each 2.5 cm. in diam., were exposed in the horizontal position, 12.5 cm. to the right and left of the fixation-point. Only the horizontal position was used in this study. Two series of experiments were performed. In Series A the standard intensity was at the right; in Series B it was at the left. These series are comparable to the experiments in positions 1 and 5 of the earlier studies.[6] The omission of the other positions, the vertical and the diagonals, permitted the inclusion of more individual experiments with every *O* at each of the positions used.

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and in bimanual activities, such as batting, sweeping, spading, and chopping with an ax. Handedness was determined in the bimanual activities by the hand "nearer the business end of the instrument." Some individuals show a bimanual preference opposite that of their unimanual; and others a divided preference. If the right hand is preferred in both unimanual and bimanual activities the individual is classified RRR; if the left hand is preferred he is classified LLL. If the right hand is preferred in unimanual activities, and the left hand in bimanual, the individual is classified RLL. If an opposite preference obtains the formula is LRR. If an individual is right-handed in unimanual activities but has a divided preference in bimanual, *e.g.* bats right-handed but sweeps left-handed, he is classified RRL. If an opposite preference obtains the formula is LLR. Cf. also J. E. Downey, Types of dextrality and their implications, this JOURNAL, 38, 1927, 317-367.

[5] Dallenbach, Some new apparatus, *ibid.*, 34, 1923, 94 f.

[6] Dallenbach, *ibid.*, 283; Burke and Dallenbach, *op. cit.*, 267.

Exposure was made by illuminating the comparison areas from behind. The duration of the exposure was controlled by a gravity tachistoscope, which was set for 1/10 sec., but the actual time used was, as measured by a Hipp chronoscope, 106 ± 1.86 sigma. The light was derived from a 500-w. concentrated filament mazda lamp, which was placed in a projection lantern behind the tachistoscope. The rays of light when uninterrupted by the tachistoscope passed through a light-proof funnel to the posterior frame of a diffusion chamber and thence, through circular windows and connecting tubes, to the ground glass windows in *O*'s booth. The intensity of the illumination was controlled by metal shutters or screens which varied the amount of light allowed to pass through the connecting tubes. One hundred screens, containing from 1 to 100 holes, every one 5 mm. in diam., and concentrated and symmetrically arranged about the center, were used to vary the intensity of the illumination. The number of holes in a screen was used to designate the intensity-value of that screen. The intensity of the standard area was, with the exception of the experiments of Series B for *Fx*, placed at 16, *i.e.* the intensity of illumination from the screen with 16 holes in it. The standard for *Fx* in Series B was 4; this shift in the standard intensity had to be made in order to bring the variable intensities required for this *O* in this series of experiments within the intensity range of our apparatus. The intensity of the variable area could be changed from 1 to 100, *i.e.* the intensity of illumination from screens with 1 to 100 holes in them.

Instructions. The following instructions were given the *Os* at the beginning of every experimental hour.

> At the signal 'Ready,' fixate the spot of light before you. At 'Now,' two areas of light will be shown. Judge which of them, if either is the more attense; that is, which of them catches your attention the more. Give the position as 'right' or 'left,' whichever the case may be. If they are equal, say 'same;' if you are uncertain, report 'doubtful.'

These instructions are similar to those given in the earlier studies. In order to assist the *Os* in establishing criteria of attensity, the following was added.

> Do not base your judgments of attensity upon intensity or form. Report which of the two stimulus-areas catches your attention first, which one stands out, is the more vivid, insistent, or clearer.

Procedure. Two series of preliminary experiments, one for each position of the standard, were made by the method of limits to determine for every *O* the approximate intensity of the variable necessary for a judgment of equality. Upon the basis of the results of these experiments 5 stimulus-values were selected for every *O* for each position of the variable, and the main experiments (Series A and B) were conducted by the method of constant stimulus differences. In each series of experiments 100 observations were taken for every *O* at every stimulus-value. The two series were conducted concomitantly in haphazard order. The *Os* never knew, therefore, which stimulus-area was the standard and which the variable. They had to judge every experiment separately and solely upon the basis of relative attensity or clearness.

When a 'doubtful' judgment was returned—and only a few in spite of the difficulty of the task were returned— the experiment occasioning it was, without *O*'s knowledge, immediately repeated.

Results. The results of this study are shown in Table I which gives for every *O* for each position of the comparison areas the amount of illumination necessary for the variable to be reported equal to the standard in attensity or clearness. These values were obtained after the 'equal' judgment had been divided between

TABLE I

SHOWING FOR EVERY *O* AND FOR EACH SERIES OF EXPERIMENTS THE AMOUNT OF ILLUMINATION OF THE VARIABLE NECESSARY FOR THE REPORT OF EQUALITY IN ATTENSITY

O	Series A (left, variable; right, standard)				Series B (left, standard; right, variable)			
	Variable		Standard	Ratio (V:S)	Standard	Variable		Ratio (S:V)
	(L)	(h)				(L)	(h)	
Fx	2.3	.620	16	2.16	4	61.1	.025	16:244
Ne	76.3	.022	16	67:16	16	2.6	.300	16:3
Sh	34.4	.041	16	34:16	16	0.8	.323	16:1

the judgments of 'right' and 'left.' Then the 50% point, the point at which *O* was balanced between the judgment 'right' and 'left,' was computed by the constant method from one of the percentile arrays.[7]

The data in Table I show that position is effective for all the *O*s, and also that it is effective for them in opposite directions. The intensity of the left stimulus-area is greater for *Ne* and *Sh*, in both series of experiments, than that of the right. This simply means that the left area, because of their predisposition toward the right, must be augmented in order to equal the right in attensity (clearness, vividness). Since both of these *O*s belong to the LLR type of dextrality, *i.e.* are left-handed in the unimanual and in most of the bimanual activities, their results may be regarded as additional evidence for the neurological theory that has been advanced in explanation of the effectiveness of position.

Fx, on the other hand, is predisposed toward the left; the intensity of the right stimulus-area is much greater, in both series of experiments with him, than that of the left. *Fx* is not, however, truly left-handed. He belongs, as we have seen, to an intermediate or mixed (R_LRL) type of dextrality. He is not ambidextrous in the sense that he uses either hand equally well in all activities, but he is ambidextrous in the sense that he performs some uni- and bimanual activities left-handed and some right-handed. His results do not, therefore, stand in opposition to the general conclusion that left-handed people have a preference for the right, for he is

[7]Since the arrays were complementary it did not matter which was used, the same liminal value would be obtained from either. We used, however, in our computations, the array of the 'left' judgments.

not to be numbered among the left-handed. At most, all that his results indicate is that *Os* of the intermediate type exhibit a positional preference similar to that of the right-handed.

The results of this experiment also show that the eyedness of our *Os* does not agree with their handedness, but that it does, curiously enough, agree with their positional preferences. *Ne* and *Sh* are left-handed, right-eyed, and predisposed toward the right; *Fx* is ambidextrous, left-eyed, and predisposed toward the left. Whether the correlation between eyedness and positional preferences, which is perfect for our *Os*, is significant we cannot say. Future experiments alone can determine that. We are, however, of the opinion, in spite of its being perfect for our *Os*, that it is not significant. We come to this conclusion because of the following reasons. (1) Earlier experimenters have found a high correlation between eyedness and handedness[8]—there is a zero correlation for our *Os*. (2) Crossed ocularity types, particularly crossed sinistro-ocularity, are rare, occurring in only 1.7% of Mill's cases[9]—and yet two of our *Os* belong to this type. (3) The eyedness of the *Os* in the earlier experiments performed in this laboratory does not, in so far as we have been able to obtain information concerning it, agree with their positional preferences. (4) Since the retinas of each eye are connected with both hemispheres, it would be difficult upon any known theoretical basis to explain positional preference by eye dominance. We have no right, to be sure, to negate our own results, but here is an instance where the results, we believe, are adventitious; where they are due to an error of sampling.

Summary and conclusions. The results of this study, which corroborate those earlier obtained, may be summarized as follows: (1) All of our *Os* (two are left-handed and the other one ambidextrous or of a mixed, intermediate type) show positional preferences. (2) The position to the right of fixation has an attentional advantage for the left-handed *Os*, and the position to the left of fixation has an attention advantage for the ambidextrous *O*. (3) Eyedness and positional preference agree—the left-handed *Os* are right-eyed and predisposed toward the right, the ambidextrous *O* is left-eyed and predisposed toward the left.

In spite of the high correlation between eyedness and positional preference obtained in this experiment, we do not believe that the relation is significant. It is, we feel, adventitious, being due to an error of sampling. We feel justified, however, upon the basis of the other results of this experiment in concluding: (a) that position is a condition of attensity (clearness, vividness) and a determinant of attention; (b) that the specific positional preference is a matter of the handedness of the *O*—right-handed *Os* being predisposed to the left and left-handed *Os* to the right; and (c) that added plausibility is given to the neurological theory advanced in explanation of positional preference.

[8]B. S. Parsons, *Left-Handedness*, 1924, 51; Lloyd Mills, Eyedness and handedness, *Amer. J. Opth.*, 8, 1925, 1-9; J. E. Downey, Types of dextrality and their implications, this JOURNAL 38, 1927, 327-333; N. B. Cuff, The interpretation of handedness, *J. Exper. Psychol.*, 11, 1928, 29; Walter Miles, Ocular dominance demonstrated by unconscious sighting, *ibid.*, 12, 1928, 113-126.

[9]Mills, *op. cit.*, 5.

APPARATUS

AN ELECTRICAL PNEUMOGRAPH

By William H. Grubbs and Christian A. Ruckmick, University of Iowa

In connection with our investigations in the field of emotion a need arose for electrically recording changes of breathing. We needed to catch any variations in the breathing processes for two reasons: first, as a check against muscular contractions which also might have left a galvanic record; secondly, because we wished to analyze the breathing curve as an accessory item in the method of expression to run parallel with our introspective accounts. It is well known that even slight movements in the respiratory tract, such as coughing or clearing the throat, leave immediate traces in terms of a galvanometric deflection. This can ordinarily be distinguished because the response is immediate while the galvanic deflection that is due to emotional or affective changes comes only after a latent period. But where a signal has not been recorded for the onset of the affective experience, breathing and other responses have to be checked off from the total record. At present, as far as the authors know, there is no reliable procedure available for photographically recording by electrical means the pneumographic techniques used in conjunction with such an apparatus as the Hathaway device or the Wechsler galvanometer.

We first tried various adaptations of variable wire-wound resistances which could be worn on the chest or abdomen but we soon discarded these even when special types of resistance-wire like small cast bars of nichrome bismuth and other high resistance-metals were used. Such devices either did not slide easily enough or left a jerky record. We finally hit upon the plan of using a carbon button similar to the button found in a telephone transmitter (Fig. 1). This button was mounted inside a fiber case on a thin strip of spring brass (A, A′). The elasticity of the spring tended to increase the ratio of external pressure and the limit of compressibility of the carbon granules (E) inside the button. The diaphragm was formed of a slightly arched disk of celluloid (B) to the center of which was attached a small disk of aluminum (C) with the appropriate electrical connections (F) to complete the circuit within the apparatus. The contact between the carbon button and the diaphragm was softened and made more flexible by a rim of absorbent cotton (D) around the button.

Wires led from this case to a battery containing two 1.5 volt cells and a Wheatstone bridge. From this box wires led to a sensitive milliammeter of 1 milliampere range which for our purposes had the shunt removed and was enclosed inside of the Wechsler photographically recording galvanometer. A small mirror was mounted on the axis of the deflecting pointer in the milliammeter. From this mirror a beam of light was reflected from a small 4.5 volt oscillograph light suitably housed to prevent leakage inside of the galvanometer box.

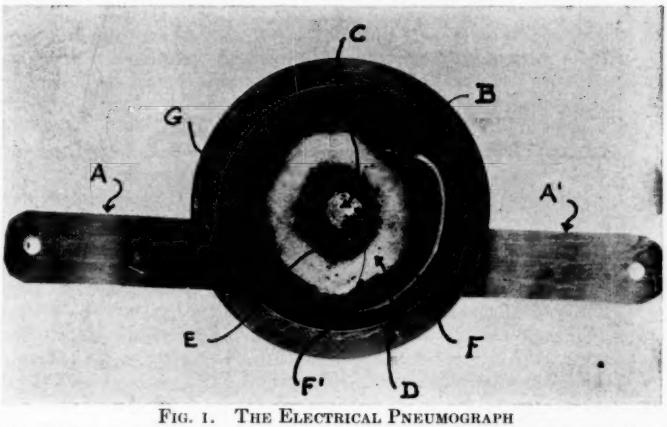

FIG. 1. THE ELECTRICAL PNEUMOGRAPH

A,A′, Strips for attaching apparatus around trunk; B, arched disk of celluloid; C, small disk of aluminum; D, rim of absorbent cotton; E, carbon granules; F,F′, wires for electrical connection, coiled at G for flexibility.

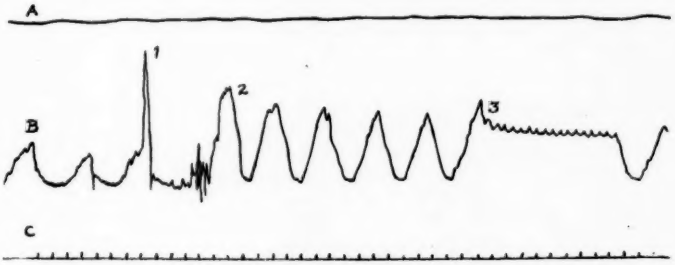

FIG. 2. PHOTOGRAPHIC RECORD FROM THE ELECTRICAL PNEUMOGRAPH

A, galvanometer line; B, respiration record: 1, exciting stimulus presented; 2, heart beat showing at crest of wave; 3, heart beat showing while breath is voluntarily held; C, time line in sec.

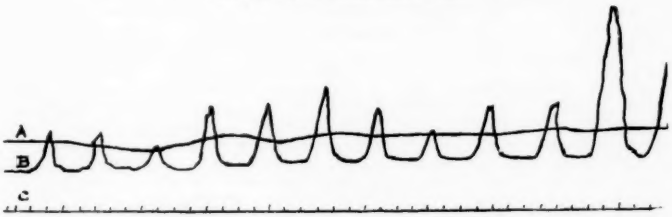

FIG. 3. CHANGES IN BREATHING AND GALVANIC REFLEX

A, galvanometer line; B, respiration record; C, time line in sec.

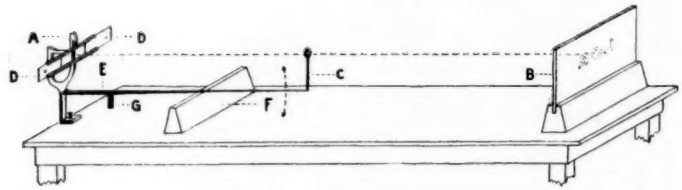

FIG. 4. THE MONOPTOMETER

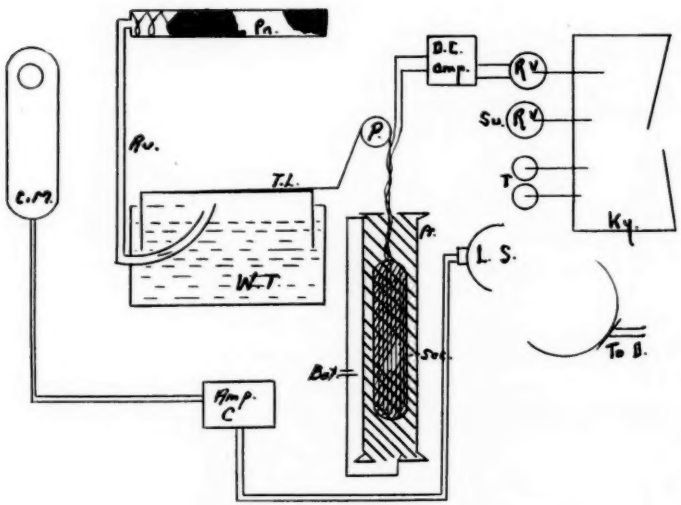

FIG. 5. DIAGRAM OF RECORDING APPARATUS

C.M., condenser microphone; Amp. C., amplifier control; L.S., loud speaker; Pn., pneumograph; Ru., rubber tubing; W.T., water tambour; T.L., tambour lever; P., pulley; Pr., primary; Sec., secondary; Bat., battery; D.C. amp., D.C. amplifier; R.V., recording voltmeter; Su., recording voltmeter to be used in circuit with subject; T., second and minute timers; Ky., kymograph; To D., to dictaphone.

At present we are standardizing the deflection so that the recorded curve is comparable with the ordinary pneumatic principle usually used with existing pneumographs. But our records so far indicate a degree of sensitivity which exceeds that obtained from the present standard equipment (Figs. 2 and 3). We are able indeed to record pulsations from the circulatory system at the crest of each wave (Fig. 2, B 2) and to get a much longer record of heart pulsations when breathing is held (B 3). The instrument can be placed on almost any part of the trunk although the curves differ for the several positions used. Some of our best results, however, were made when it was placed on the floating ribs of the left side.

The entire arrangement is compact, easily portable and is especially adapted to use where the *O* is advantageously removed to a room far from the recording apparatus. For simple arrangements in the drill laboratory and for demonstrational purposes the standard pneumograph will still have points of merit but where careful research is planned and especially where accurate photographic records are desired as happens in so many of our investigations the electric pneumograph ought to find a place in our laboratories. The whole equipment ought not to cost more than $15, not including the Wheatstone bridge.

THE MONOPTOMETER: A NEW DEVICE FOR MEASURING EYE-DOMINANCE

By F. H. Lund, Temple University

The fact of eyedness, *i.e.* dominance of one eye over the other in visually directed movements, seems to have been recognized long before any attempt was made to measure or determine it objectively. As early as 1861, Humphrey referred to "the correspondence of function between hand and eye."[1] Again, in 1881, LeConte called attention to the relation which exists between eyedness and handedness.[2] Since then a number of other writers and investigators have dealt with the subject.

The first attempt to introduce quantitative measures was made by Parson, in 1924, who reported the results of tests in which he used the manoptoscope as his measure of eyedness.[3] This instrument, devised by Parson, consists of a conically-shaped sighting tube and an exposing device which is placed at a distance of about 2 ft. from the *S*. In the center of the exposing device is placed a fixation-disk, with an *R* to the right of the disk and an *L* to the left. If the *S* is right-eyed and fixates the disk with the right eye he will, with the left eye, see the *R*. The *L*, on the other hand, will be seen with the right eye if the *S* fixates the disk with the left eye.

In 1930, Cuff published an account of the manoptometer,[4] which is an improvement upon Parson's manoptoscope, in that the new apparatus permits (a) of a more accurate regulation of the distance between the *S* and the exposing

[1] G. M. Humphrey, *The Human Hand and the Human Foot*, 1861, 8.
[2] J. LeConte, *Sight*, 1881, 107.
[3] B. S. Parson, *Lefthandedness*, 1924, 7 ff.
[4] N. B. Cuff, A manoptometer, this Journal, 42, 1930, 639.

device, and (b) of the regulation of the distance between the fixation-disk and the objects seen by the non-dominant eye. In as much as the sighting tube is retained in its original form, it does not, however, seem to us that Cuff has obviated the chief objections to Parson's device. These objections are (1) the conditions under which the *S* is required to fixate are highly artificial, since vision is limited to what may be seen through the circular aperture, and the *S* cannot be guided—as he normally would be—by objects seen in indirect vision; (2) with vision thus restricted, it becomes largely a chance matter as to which eye first sights, or gets into line with, the object of regard—this being the case, there is a good deal of room for chance errors, particularly in the case of *S*s manifesting mild dominance; and (3) the eyes of the *S* being shielded from view by the sighting tube *E* is unable to tell whether the *S* is following instructions. Moreover, in determining the form of dominance present, the experimenter must depend almost entirely upon the *S*'s own report.

The *monoptometer* devised by us (see Fig. 4) was constructed with a view to meeting these objections. It will be seen from the figure that the circle (C) mounted upon the bar (E) extending from the head-rest (A) is on a level with the slides (D) and the fixation-disk (B). Thus, the *S*, seated at the head-rest, may be instructed to bring the circle into line with the fixation-disk. In doing so the *S* is required to keep both eyes open, and to manipulate the bar (E), and thus the circle (C), by the handle (G), which is grasped with the favored hand. The experimenter standing opposite the circle, is able to follow the operations of the *S*, and can tell immediately, from the point at which the extension bar (E) comes to rest, which is the dominant eye. If it stops on the right side of the median line (a line determined beforehand and marked off on the bar rest (F)), the *S* is right-eyed. If it stops on the left side, the *S* is left-eyed. Through repeated trials in which the *S* is required to bring the mounted circle into line from a position well to the right, or well to the left—the procedure alternating—it is possible to determine both the *kind* and *degree* of eye-dominance possessed by the *S*. If dominance is of the less decided or milder form there is apt to be less consistency in the point at which the bar is brought to rest in successive trials.

Since the apparatus is less a "measure of handedness and eyedness" than it is a "measure of one-eyedness," the term, *monoptometer*, seemed more appropriate than the term, *manoptometer*, used by Cuff.

The slides, (D), are not essential to the experiment. They are introduced merely as a convenient means of demonstrating to the *S* which eye is used in sighting. In the absence of these slides, a book, or card, may be held in front of the dominant eye, so that the *S* can see for himself which eye he is using.

In order to secure a comparative measure of the monoptometer and the manoptoscope, 526 *S*s were tested for eyedness, using first the one instrument and then the other. The monoptometer tests yielded 69.8% right-eyed, 25.5% left-eyed, and 4.6% binocular and ambiguous. The manoptoscope tests yielded 63.7% right-eyed, 20.4% left-eyed, and 15.9% binocular and ambiguous. In the case of the latter, the decrease in the number of right-eyed and left-eyed, and the decided increase in the number of binocular and ambiguous, may be attributed to the special conditions of testing when the manoptoscope is used. Also, to the fact that, with this instrument, it is largely a chance matter as to which eye first

gets into line with the disk. Then, again, the size of the circular aperture in the sighting tube, and the distance at which it is held, permits the *S* to bring together the images of the two eyes, or, at least, to cause an overlapping of the images within the limits of the disk.

In view of the facts we have considered, the monoptometer would seem to be superior to other devices for measuring eye-dominance, first, in that the test conditions are more objective, less artificial, and more nearly in accord with the requirements of ordinary vision; secondly, in that the form of dominance can be determined without the aid of the *S*'s report; and, thirdly, in that it is possible to secure quantitative results, not only with respect to *kind* of dominance, but also with respect to *degree* of dominance.

COMBINING THE BREATHING UNDAE OF SPEAKER AND LISTENER WITH THE DICTAPHONE RECORD OF THE SPEECH

By Lyman Spicer Judson, Alabama Polytechnic Institute

The writer has pointed out that the breathing of the listener is affected by the stimuli of the heard words.[1] It has also been noted by MacDougall,[2] Gamble,[3] Wycozoikowska,[4] Clark,[5] Golla and Antonovitch,[6] and others that the breathing of the speaker not only varies during thinking, silent reading, whispering, and overt speech, but that it probably varies with different kinds of thinking, reading, and speaking.

The apparatus described provides an objective means of recording the breathing of the speaker and listener, and of furnishing an accurate identification of the word-breathing relationship. Essentially, the equipment makes it possible to record every breathing movement of the thorax (and abdomen, if required) and to record at the same time the word or words which accompanied the breathing movements. The two are closely correlated by identical timing devices.

Fig. 5 shows the spring pneumograph worn by the speaker.[7] This impinges a column of air directly to a knife-edge, water tambour. The tambour lever operates, over a pulley system, the moving coil of the Electrodynamic Recorder. This latter unit consists of a light movable coil cutting lines of force in the field of an energized primary helix. The induced current set up is amplified and controlled through a D.C. amplifier for convenient operation of a zero-center, dead-beat recording voltmeter. The voltmeter lever writes directly on the kymograph

[1]L. S. Judson, Objective studies of the influence of the speaker on the listener, *J. Expression*, 4, 1929, 1-11.

[2]R. MacDougall, The physical characteristics of attention, *Psychol. Rev.*, 3, 1896, 158-180.

[3]E. A. McC. Gamble, Attention and thoracic breathing, this Journal, 16, 1905, 261-292.

[4]A. Wycozoikowska, Theoretical and experimental studies in the mechanism of speech, *Psychol. Rev.*, 20, 1913, 448-458.

[5]R. S. Clark, An experimental study of silent thinking, *Arch. Psychol.*, 7, 1922, (No. 48), 1-101.

[6]F. L. Golla and S. Antonovitch, The respiratory rhythm in its relation to the mechanism of thought, *Brain*, 52, 1929, 491-509.

[7]The Electrodynamic Recorder may be used directly with the air-pressure system of Golla and Antonovitch.

paper. Two of these combined air-recorder units may be used in obtaining both thoracic and abdominal records. Any convenient number of these combinations may be used to obtain the breathing undae of the speaker and one or more listening *Ss*. For long speeches a switch in the recorder circuit between the amplifier and the voltmeter permits almost instantaneous change from one kymograph to a fresh kymograph record.

The speech to be recorded is passed through a condenser microphone and amplifier to a loud speaker. The loud speaker is in close proximity to the time-markers indicating seconds, minutes, or any other convenient interval. In addition to marking the kymograph record, these timers are adjusted to click loudly or to ring a small bell. These audible time-markings are received, superimposed on the speech, by the dictaphone. Thus the breathing of the speaker, as recorded on the kymograph, is timed by the identical timer which times the speech on the dictaphone record. This provides an objective record of the cause-and-effect relationship of speaking and breathing, which may be studied with the assurance that it shows the original correlation of the two factors.

For long speeches, continuity of the dictaphone record may be assured by using two dictaphones and over-lapping the records.

The use of this apparatus makes it possible to apply objective measurements to changes in breathing on the part of the speaker, due to (a) internal conditions and (b) external conditions (*e.g.* applause, hissing, which can be directed by accomplices of the experimenter), and to changes in breathing on the part of one or more listeners, due to the influence of the speaker or of the crowd. The utilization of distant recording devices, such as the Electrodynamic Recorder and the microphone, makes it possible to measure changes in the actual audience-situation with laboratory accuracy. The recording may be done at any convenient distance from the audience.

With the close coöperation of the speaker, and by having the listeners under observation scattered in the audience, it is possible very nearly to duplicate the normal audience-situation.

APPARATUS NOTE

A Simple Device for Producing an Illusion of Seen Movement

The device described here is essentially a modification of the well-known barber-pole illusion.[1] A cylinder of any stiff material is spirally striped or grooved to produce the effect of a barber-pole. A large disk or washer is inserted tightly over the cylinder and fastened at about the center of its length. The dimensions of the parts depend on the use of the device. For demonstration purposes a rod 18 in. in length and 2 in. in diam. gives satisfactory results. This is inserted through a washer 1 in. thick with an external diameter of 4 in. and a hole cut as close as possible to the diameter of the cylinder. Rotation of the device results in an apparent movement of the washer along the axis of the cylinder. This movement can be eliminated only by establishing visual points of anchorage on a stationary background.

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Tufts College
H. Barry, Jr.

[1] Cf. J. P. Guilford, Illusory movement from a rotating barber pole, this Journal, 41, 1929, 686 f.

NOTES AND DISCUSSIONS

A Case of Apparent Blue-Blindness

R is a young woman 19 years of age and next to the eldest of a family of 6 children consisting of 4 girls and 2 boys. She has had, as far back as she can remember, difficulty in the perception of certain colors. So far as she knows no one else in her family has had such trouble, nor has she herself had any accident affecting her eyes. Her eyes are grayish blue in color and appear normal to the ordinary observer. When asked her favorite color she replied, "blue." She has never needed to wear glasses.

R was first given the Nagel test.[1] In this test she named all colors correctly except in the case of Plate B-1, in which she called the yellowish brown spots green and the brown spots a darker green. Since this test showed *R* not to be a case of ordinary red-green blindness, an attempt was made to determine whether she was a case of so-called blue-blindness. This was done by means of tests made especially for the purpose.

TABLE I

Showing *R*'s Ability to Distinguish Letters in a Color-Confusion Test

Plate	Color	Letter	Response
1	Y-O St. and V-R St.	P	+
2	G-Y St. and G St.	H	+
3	Ro-V St. and G-Y St.	O	+
4	G-B St. and B-G St.	V	−
5	Y-G St. and B-V Tint 1	F	+
6	Y Tint 1 and G-B Tint 1	E	+

Plates were prepared of bits of paper of different colors stuck on a white background of ordinary drawing paper. A letter was made up of particles of one color surrounded by particles of another color interwoven about the letter. The letter could easily be discerned by a person of normal color vision. The colors used in making up the plates were Milton Bradley standardized colors. The bits of color were about 1/8 in. across and the letters about 1 1/2 in. high. The plates were held about 15 in. from *R*.

The construction of the plates used in the first tests for blue-blindness is shown in Table I. In this table the colors and the letters used in the various plates are shown in the second and third columns, while *R*'s responses are indicated in the fourth. The colors are abbreviated as follows: Y-O means Yellow-Orange; G-Y means Green-Yellow; Ro-V, Rose Violet; G, Green; G-B, Green-Blue; B-G, Blue-Green; Y-G, Yellow-Green; B-V, Blue-Violet; Y, Yellow; and St. means standard. Every plate was first presented to a normal *S* (who indicated by a nod of the head if she perceived the letter) and then to *R*, who gave an oral response naming the letter or else indicating that she could perceive no letter. A plus sign in the

[1] W. Nagel: Tafeln zur Untersuchung des Farbenunterscheidungs-Vermögen, 9th ed., J. F. Bergman, Weisbaden, Germany.

fourth column indicates that *R* was able to distinguish the letter; a minus sign that she could distinguish no letter. The tests were presented in good daylight. The results show that the letter V in G-B St. on a B-G St. background alone passed unnoticed.

Further tests were constructed on a similar plan for the purpose of finding out whether *R* could distinguish between shades of blue and gray. Five blues (1–5) and 6 grays (A–F) were used. In Table II correct designations for each of the blues (taken from the Milton Bradley Standard Color Chart) appear horizontally across the top of the table, while names of the grays according to the same chart are given in the vertical column at the extreme left. A plate composed of Gray A and Blue 1 will be designated as A (1), one composed of Gray B and Blue 2 as B(2) etc. The letter used in each plate is shown in the first vertical column under each caption; the responses to the various plates as they were presented to *R*, in the next two columns. A plus sign in the second column indicates that *R*

TABLE II

SHOWING *R*'S ABILITY TO DISTINGUISH LETTERS AND COLORS IN A COLOR-BRIGHTNESS CONFUSION TEST

	Blues									
	(1)		(2)		(3)		(4)		(5)	
Grays	Shade 1		Tint 1		Tint 2		G-B Tint		G-B St.	
A. Very dark, almost black	H	+ +	X	+ +	L	+ −	U	+ −	A	+ +
B. Dark, cool	P	+ −	K	+ −	M	+ −	L	− −	A	+ +
C. Light, warm	E	+ +	S	+ +	T	+ −	J	+ −	O	+ −
D. Light, neutral	N	+ +	R	+ +	A	− −	A	+ −	E	− −
E. Green-gray, light	N	+ +	U	+ −	H	+ −	V	+ −	L	+ +
F. Light, cool	F	+ −	Y	+ −	S	− −	U	+ +	R	− −
Failure, letters		0		0		2		1		2
Failure, colors		2		3		6		5		3
Failure, brightness						2		1		2

was able to distinguish the letter in the plate and a minus sign that she was unable to do so. In the third column a plus sign indicates that *R* distinguished the colors and a minus sign that she did not. (When *R* was able to distinguish the letter she was asked if the letter and background were the same color). Reading under (1) we should have then the following facts shown: in Plate A(1), *R* was able to distinguish the letter (H) and did not confuse the colors which appeared on the plate; in Plate B(3) *R* was able to distinguish the letter (M) but she confused the colors. The responses marked by one minus sign show that *R* was able to perceive the letter but was unable to distinguish between the colors. The responses marked by two minus signs show that she was unable to perceive the letter and hence could not distinguish between the two colors as to hue or brightness. The table shows that 19 out of 30 plates (63.3%) gave a confusion of colors, while 14 out of 30 responses (46.6%) showed confusion of colors but ability to perceive the letter, indicating confusion of colors but not of brightness. Five out of 30 responses (16.6%) show inability to distinguish the letter, *i.e.* confusion of both color and brightness.

Further tests were made to study *R*'s reactions to shades of green and yellow and to blue and green. Table III shows the construction of the plates and *R*'s responses. The second and third columns show the colors of which each plate is composed and the letter used. Columns 4 and 5 give the responses, showing *R*'s success or failure in distinguishing the letters and the colors. Thus, reading from the table, we see that in Plate 1 made up of G St. and G-Y St. *R* was unable to distinguish the letter J which appeared on the plate, hence the two colors appeared of the same hue and brightness. In two cases she was unable to perceive the letter and in three cases the colors appeared the same. In one case *R* was able to distinguish the letter but confused the colors, showing that she distinguished the letter by difference in brightness.

TABLE III

SHOWING *R*'S ABILITY TO DISTINGUISH LETTERS AND COLORS IN A COLOR-BRIGHTNESS CONFUSION TEST INVOLVING CERTAIN SHADES OF GREEN AND YELLOW

Plate	Colors	Letter	Ability to Distinguish Letters	Colors
1	G and G-Y	J	−	−
2	G-Y and Y-G	H	+	−
3	Y and G	N	−	−
4	G-B and G	L	+	+

Discussion. The first test for what is called blue-blindness (Table I) shows that, in all but Plate 4, *R* is able to distinguish the letter. Failure on Plate 4 indicates confusion as to both color and brightness between G-B St. and B-G St.; but the extent of the confusion of colors is not clear from this test. Table II, however, shows most clearly the nature of *R*'s difficulty. In nearly half of the responses she could distinguish between grays and blues as to brightness but not as to hue. In no case was she able to distinguish the hue of Blue Tint 2 from any of the grays nor could she distinguish it either by hue or brightness from light neutral gray or light cool gray.

She had less difficulty, to be sure, with Blue Shade 1, and Green-Blue St. Blue Shade 1 she could distinguish from dark cool gray or light cool gray as to brightness, though not as to hue. In no case did she confuse Blue Shade 1 with a gray both as to hue and brightness.

In nearly two-thirds of all cases, however, *R* confused the hues of the blues and grays, showing the nature of her case clearly. She is, as we believe, a case of true partial color-blindness and not just of color weakness. She was unable to distinguish either by hue or brightness Blue Tint 2 from light neutral gray or from light cool gray; Green-Blue Tint 1 from dark cool gray; Green-Blue St. from light neutral gray or light cool gray.

It will be noted that 3 of the 5 confusions involving both brightness and hue occur when the blues were greenish in color. This probably has some significance. It will be noted also that 4 of the 5 confusions involve light neutral gray or light cool grays. This shows a tendency to confuse the lighter grays and the greenish blues both as to hue and brightness. Every gray, however, had its share of confusion with some one or other of the blues. Very dark, almost black gray was confused as to hue with the least number of blues.

The results in Table III shows that *R* has difficulty also in distinguishing between certain shades of green and yellow. She can see no difference in brightness nor hue between Green St. and Green-Yellow St. or between Yellow St. and Green St., whereas Green-Yellow St. and Yellow-Green St. appear the same as to hue but not as to brightness.

It may be inferred from the results of these tests that *R*'s case is a peculiar one of partial color-blindness. She is unable to distinguish between many shades of gray and blue, and between certain blues; nor can she distinguish between certain greens and yellows. The spectrum probably appears to her as different saturations and brightnesses of red and green, most yellows appearing greenish and the blues greenish or grayish. It is difficult to give a theoretical explanation over and above the suggestion that there is some abnormality in the rods and cones of the retina which abnormality has no doubt been inherited. There could well be further profitable study of *R*'s case and the writer would be glad to put any investigator in touch with her.

Columbia University. C. E. HUNTINGTON.

AN UNUSUAL CASE OF LOSS OF HABIT

A pair of gray squirrels which came under my observation regularly performed two successive responses on hickory nuts, each of which indicated discrimination of the apical end. Experimentation with these responses resulted in the loss of one and the retention of the other by each squirrel. So far as I know, this is a new phenomenon, and not without interest.

The squirrels, a male and a female, were received from a collecting station in Texas when about half grown, and kept under laboratory conditions. They never became gentle enough for handling. Except where otherwise noted, they were caged together.[1]

From the time the squirrels were first observed, they turned nuts over and over before opening them. During this examination the apical projection was bitten off, while the basal projection was not touched.

The opening of the hickory nuts was also found to proceed in a manner which required discrimination of the apical end. The squirrels split the nuts into halves by gnawing the flattened sides at the base of the apical projection. The kernels were then easily procured by chipping away the thin shell at the basal end. All the empty shells, which were in almost perfect halves, showed gnawing at the apical end and chipping at the basal end. An examination of the shells of 85 consecutive nuts revealed not a single exception.

To determine whether the discrimination of the apical end was visual, the squirrels were given hickory nuts in a dark room. They opened the nuts without hesitation in the same manner as before.

The animals were returned to daylight and nuts were presented from which the projections had been filed off so that both ends were uniformly smooth. This destroyed the habit; the squirrels seldom found the apical end; they gnawed at random on the surface of the nuts. Of a total of 23 filed nuts, only 2, or 8.7%, were split into halves as before. The empty shells of the other nuts were very

[1] I am indebted to Ohio State University for the original purchase of the squirrels and for permitting them to be forwarded to Michigan State College, where the observations were made.

much gnawed, showing that much more than ordinary time and energy had been required to open them. The sensory control of the discrimination must therefore have been either tactual or olfactory or both.

When fed normal nuts the following day, the squirrels bit off the apical projection as before. This they continued to do throughout the period of experimentation. Although the animals were not always observed in the act, examination of the shells showed that the apical projections had been removed from nearly all opened nuts, while the basal end was not scratched. In many cases the squirrels were observed in the performance of the response.

However, the squirrels did not resume their old habit of splitting the nuts, but gnawed the surface of the nuts indiscriminately as they had done with those that had been filed. Often a large section of a nut was gnawed away, and frequently even after such exertions not all of the meat was secured. The few cases in which the apical end was found seem to have been the result of chance. The habit was completely lost.

For over four months the animals were fed the same kind of hickory nuts as before. The only other kinds of food given were some other nuts mentioned later, and some vegetables. The shells of the opened hickory nuts were examined at intervals to determine the percentage of the nuts which had been split into halves by gnawing at the apical end only.

From March 21, the day after the sensory control tests were made, to April 15, 215 nuts were opened, of which 8.7% were opened "correctly". From April 15 to June 6, 309 were opened with 7.1% correct. From June 6 to June 22 the squirrels were fed no nuts whatsoever. Following this rest, from June 22 to July 5, 172 nuts were opened and 10.4% were correct. Of the 226 nuts opened from July 5 to July 30, 13.7% were correct. The experiment had to be discontinued at this time.

From June 22 onward the two squirrels were kept in separate cages. The individual records may be of interest in addition to the totals given above. The male opened 145 nuts, of which 9.7% were correctly opened; the female opened 253, of which 13.8% were correct.

It will be recalled that before the filed nuts were presented the squirrels had opened 100% of the nuts correctly. Of the 23 filed nuts, 8.7% were correct. When 932 normal nuts had been opened after the filed nuts, the correctness was only 13.7%. This practice covered a period of over four months. The original habit must have been acquired much more rapidly, for the squirrels could have been little more than four months of age when they were obtained.

Before the filed nuts were given, forty each of hazel nuts and of almonds had been presented to the squirrels, and this was repeated after the former manner of opening hickory nuts had been lost. In both cases the nuts were invariably opened from the apical end, which was the sharper end. Nuts with apical ends less different from the basal, such as English walnuts and acorns, were usually but not uniformly opened from the apical end, both before and after the presentation of the filed hickory nuts. The loss of habit was therefore specific to hickory nuts.

In order to throw some light upon the distribution of the apical method of opening nuts, 180 hickory nutshells, apparently opened by squirrels, were

gathered at random in a forest. Only 34.7% of these were split in half. Either this is a learned response acquired by relatively few squirrels or it is an instinctive response lost by a large number of squirrels. The first alternative seems the more probable.

The two squirrels here studied accepted and opened wormy nuts of any kind as readily as good ones. This fact supports an earlier observation of the author that the rejection of wormy nuts is very probably a learned response.[2]

It is a matter of common knowledge that when the difference between two stimuli is reduced until the discrimination disappears, the animal may subsequently fail to respond accurately to stimuli which were formerly differentiated very easily. In this experiment that is what happened to the "opening" response when the two ends of the nut were made indistinguishable. However, in the present case, discrimination of the end of the nut was indicated by the removal of the projection as well as by the act of opening the nut. The habit of removing the apical projection was not lost. The experiment shows that when discrimination responses break down the ability to perceive the difference between the two stimuli is not necessarily lost, for the persistence of this ability is witnessed by the second discrimination response. But the difference no longer leads to the customary response. The change has been associational rather than sensory.

University of Virginia WAYNE DENNIS

A SUMMARY OF CERTAIN FACTORS IN CURRENT INVESTIGATIONS ON LEARNING AND MEMORY

This note represents an attempt to summarize certain trends of experimentation on learning and memory reported during the years 1925-1930 inclusive. Slightly more than 7% of the total number of pages issued during the period by 22 psychological publications has been devoted to studies of this type.[1] A survey of this material brought out certain facts pertaining to (*1*) the size of the groups employed, (*2*) the kinds of subjects experimented upon, and (*3*) the age of the human subjects used. In obtaining the data for the following tables, the journals cited were read, and any reports of an experimental nature, *i.e.* those in which factors were systematically varied in an attempt to determine their effect upon learning and memory, were included. There was a decided tendency for studies to be titled 'experimental' when they were quite obviously statistical.

[2]W. Dennis, The rejection of wormy nuts by squirrels, *J. Mammalogy*, 2, 1930, 195-201.

[1]The journals examined totaled 53,671 pages of psychological literature of which number 3,937 pages dealt with experimental studies of learning and memory. The following journals were included in this summary: (1) *Amer. J. Psychol.;* (2) *Arch. Psychol.;* (3) *Australasian J. Philos. and Psychol.;* (4) *Brit. J. Psychol.;* (5) *Brit. Psychol. Monog.;*(6) *Univ. Calif. Pub. Psychol.;* (7) *Genet. Psychol. Monog.;* (8) *Univ. Iowa Stud. in Psychol.;* (9) *J. Abnorm. Psychol.;* (10) *Psychol. Clinic;* (11) *J. Comp. Psychol.;* (12) *J. App. Psychol.;* (13) *J. Exper. Psychol.;* (14) *J. General Psychol.;* (15) *J. Educ. Psychol.;* (16) *Mind;* (17) *Mental Hygiene;* (18) *Ped. Sem.;* (19) *Psychoanalytic Rev.;* (20) *Psychol. Rev.;* (21) *Psychol. Bull.;* and (22) *Psychol. Monog.*

Such reports were not included in the present summary, nor were a few lengthy theoretical articles supported by meager and incidental experimentation.

(*1*) *Size of groups.* The total number of *S*s used in any experimental program is of little consequence, the important consideration is the number of *S*s used per experimental group. Table I is, therefore, based upon the number of *S*s used per group, and gives for every year of the 6-yr. period the percentage of the total number of groups used which falls within certain designated class limits.[2] More than 23% of the groups numbered less than 11 *S*s. It would seem that such studies are worse than useless for comparative purposes. In a number of investigations college classes were used for research purposes with the apparent implication that any two of them were sufficiently equated for experimental comparisons. This assumption is gratuitous. The equation of experimental and control groups is quite unsettled, especially in the field of infra-human study. Researchers using animal *S*s rarely make an attempt toward preliminary equation of groups. Labeling conclusions tentative does not seem to be sufficient justification for the use of groups which are compared without being subjected to some sort of preliminary equation.

TABLE I

PERCENTAGE OF THE TOTAL NUMBER OF GROUPS USED EACH YEAR WHICH FALL WITHIN CERTAIN CLASS LIMITS

Year	1-10	11-20	21-30	31-40	41-50	51-60	61-70	71-80	81+
1925	4	22	23	7	6	8	1	9	20
1926	25	31	19	6	1	10	1	0	7
1927	20	23	8	8	9	1	2	7	22
1928	30	12	14	6	1	7	6	2	22
1929	34	15	13	6	12	2	2	3	13
1930	29	27	24	5	6	3	1	1	4

(*2*) *Kinds of Ss.* It is of interest that so few studies have been reported upon *S*s other than vertebrates. One who wishes to approach the problems of learning and retention phylogenetically must progress very sketchily until he reaches the fishes. Table II partially summarizes the present status of affairs with respect to this factor by giving the percentage of the total number of learning and memory studies which was concerned with humans, with rats and mice, and with all other animals. The major part of the work dealt with humans to the almost entire neglect of all phyla except the vertebrate. Less than 1% of the investigations reported involved the use of invertebrates.

(*3*) *Age of the human Ss.* Theoretically, the problems of learning at any one developmental level are neither more nor less important than those at any other. For an all inclusive and systematic psychology of the growth of human learning ability it is essential that there be at least a large number of studies for each age. That this situation does not prevail has frequently been noted. There are many studies of the learning and retention of college and graduate students, but experi-

[2]The percentages in Table I involve more groups than there were studies reported upon. This is due to the fact that many single investigations involved two or more groups.

mental investigations of the ability of pre-school and primary grade children are relatively scarce. Table III gives the percentages of the total number of experimental studies concerned with humans at the different age levels. The limits of the age groups cited are arbitrary, but are nevertheless most consistent with the designations used in the studies reported. Over 62% of the studies conducted were performed on adults.

TABLE II

PERCENTAGE OF THE TOTAL NUMBER OF LEARNING AND MEMORY STUDIES DEVOTED TO DIFFERENT ANIMALS

Ss	1925	1926	1927	1928	1929	1930	Mean for 6 years
Human	68	51	56	50	56	71	58.7
Rats and mice	23	19	33	34	40	25	29.0
All others	9	30	11	16	4	4	12.3

TABLE III

PERCENTAGE OF THE TOTAL NUMBER OF HUMAN STUDIES PERFORMED ON DIFFERENT AGE GROUPS

Group	1925	1926	1927	1928	1929	1930	Mean
Pre-school	11	12	16	6	11	9	10.8
Grades I-IV	6	8	5	12	4	2	6.1
Grades V-VIII	21	5	10	21	6	7	11.7
High school	3	13	4	11	15	10	9.3
Adults	59	62	65	50	63	71	62.1

The following three statements summarize briefly the above tables: (1) Of all the groups used in experiments on learning and memory, 23.7% numbered less than 11; (2) 58.7% of such studies were performed on humans, with less than 1% being devoted to invertebrates; (3) 62.1% of the studies involving humans were performed on adults, with but 17.8% concerning themselves with school children.

De Pauw University STEPHEN M. COREY

A NOTE ON "A NEGLECTED POSSIBILITY IN FREQUENCY THEORIES OF HEARING"

In a recent number of this JOURNAL Kreezer brings forward what he regards as a "neglected possibility" in auditory theory.[1] It happens, however, that his proposal is no more nor less than a statement of the volley theory, and as such is far from neglected in present-day discussions.

Kreezer throws aside our suggestions regarding the volley theory because, as he asserts, we assume nerve-fiber response inevitably to occur at a particular phase in the wave of stimulation. We have made no such assumption. On the contrary, in Fig. 2 of our article where the volley theory is represented,[2] we have pictured the fibers as showing variability in their response and hence producing by a piling up of discharges a form of distribution curve with a maximum corresponding to the point of maximum stimulation.

[1] G. Kreezer, A neglected possibility in frequency theories of hearing, this JOURNAL, 43, 1931, 659-664.

[2] E. G. Wever and C. W. Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 365-380, esp. 376 ff.

The assumption of this type of distribution of discharges is necessary if the volley theory (or the telephone theory either for that matter) is to account for the results that we have obtained experimentally, for, contrary to Kreezer's supposition, our amplifier and telephone receiver system will not distort to the extent of giving a sine-wave type of response to a signal that departs at all widely from it. His suggestion that the nerve-discharges are distributed uniformly throughout one half-cycle of each wave is therefore not possible, for such a "square wave" type of response would be distinctly noisy in its acoustic effect, and would not be adequate to the clear transmission of speech. The use of high-frequency oscillographs, which he proposes for testing this point, is thus hardly necessary; yet if he had examined our reports carefully he would have learned that we have made use of the cathode ray oscillograph in these experiments, though primarily in measurements of intensity where such instruments indeed excel the telephone receiver.[3] We may say here, in order to leave no chance for misunderstanding, that our oscillographic studies check with the work with the telephone receiver in excluding such a type of response as Kreezer suggests.

Incidentally we add that Gasser did not "develop" the cathode-ray oscillograph, nor did Rosenberg develop the bifilar oscillograph, though each deserves the credit of using these instruments in nerve studies; and that the discussion of quanta of intensity, which Kreezer enters into (with certain arithmetical peculiarities), is pointless in the utter absence of experimental results indicating such quanta in this or any other sense-field. For, indeed, it is a serious stumbling-block of "straight telephone" theories, along with the simple place theories, that they require that quanta, either of intensity or of pitch, be assumed despite the absence of experimental indications of them;[4] and it is a particular merit of the volley theory that it leads to no such assumption.

Princeton University

E. G. Wever
C. W. Bray

The Thirty-Ninth Annual Meeting of the American Psychological Association

The thirty-ninth annual meeting of the American Psychological Association was held at the University of Toronto, Toronto, Ontario, on Thursday, Friday, and Saturday, September 10, 11, and 12, 1931. Registration figures show a total of 448 persons in attendance classified as follows: 128 members, 166 associate members, 29 newly-elected members, and 125 persons not affiliated with the Association.

At the business meeting, 5 new members and 264 associate members were elected. The Secretary announced that the mail ballot on the question of alternating Annual Meetings east and west of the Appalachian Mountains showed 655 favoring such policy and 161 opposing, and that the mail ballot on the question of September *vs.* December as the time of meeting showed 581 favoring September and 238 favoring December. The By-Laws were amended to provide

[3] Wever and Bray, The nature of acoustic response, *J. Exper. Psychol.*, 13, 1930, 380.

[4] For a discussion of this matter of quanta, see E. G. Boring, Auditory theory with special reference to intensity, volume, and localization, this Journal, 37, 1926, 160.

for holding future Annual Meetings during the first two weeks of September. Another change in the By-Laws moves the date for closing applications for Associate Membership from March 15 to July 1. It is to be noted that the date for closing applications for members and for transfers from associate to member remains March 15. It was voted to accept the invitation of Cornell University to hold the fortieth annual meeting at Ithaca, N. Y. on September 8, 9, and 10, 1932 and to appoint Professor Madison Bentley as a member of the Executive Committee for 1931-32. The Council of Directors was authorized to make arrangements for a meeting in Chicago in September 1933. Newly-elected officers are: President, Walter R. Miles; Directors, Calvin P. Stone and Edward C. Tolman; Secretary, Donald G. Paterson; Division of Anthropology and Psychology, N.R.C., K. S. Lashley and Joseph Peterson; S.S.R.C., Carl C. Brigham; A.A.A.S. Harvey A. Carr and Walter F. Dearborn. The appointment of Herbert S. Langfeld as editor of the Psychological Monographs was ratified. Leonard T. Troland, Clarence E. Ferree, and A. T. Poffenberger were appointed to represent the Association on the Inter-Society Color Council. J. McKeen Cattell was appointed as the Association's representative at the Seventh American Scientific Congress to be held in Mexico City, Mexico, in February 1932.

The program represented a departure from previous practice in so far as no scheduled Round Tables were included. Ten sessions for formal papers were scheduled, designated as A or B programs, and seven additional sessions, designated as C programs, were scheduled for briefer reports, titles only listed in the Abstracts. Forty-eight papers were presented by members and associates in the A and B programs and fifty-seven ten-minute reports were presented in the C programs. Without making a detailed quantitative analysis of the program in comparison with previous programs, one gains the impression that the bulk of present-day experimentation, as reflected in the Toronto papers, is concerned with what Woodworth designates as the "psychology of performance," the subjects being rats, primates, new-born infants, pre-school children, college students representing adults, and the senescent. There were very few papers on applied or technological psychology.

On Thursday morning the Section of Clinical Psychology held its annual meeting followed by presentation of six papers. The newly-elected officers are: Chairman, Fred Kuhlmann; Secretary, Luton Ackerson; and other members of the Executive Committee, T. C. Hegge, M. R. Trabue, and F. N. Maxfield.

On Thursday afternoon at 4:00 p. m. an informal conference on studies related to the broadcasting of psychology was sponsored by the Committee on Psychology appointed by the American Psychological Association to coöperate with the National Advisory Council on Radio in Education. The attendance and discussion clearly indicated wide-spread interest in the educational and research opportunities afforded by the radio.

The University of Toronto was host at a delightful tea held in the Quadrangle of Hart House on Friday afternoon. The tea was followed by an open house affording delegates an opportunity to visit all departments of Hart House which is a "cultural center" in the broadest sense of the term.

On Friday evening, the Reverend Dr. H. J. Cody of the Board of Governors of the University of Toronto cordially welcomed the Association Membership and

gave a brief history of psychology at Toronto, including an account of its present status as a subject of instruction, research, and state service. This was followed by the Presidential Address, "The Psychological Study of Behavior," by Dr. Walter S. Hunter in which it was argued that psychology seeks to describe and explain extrinsic behavior whereas physiology deals with intrinsic behavior. Extrinsic behavior is not dependent upon a specific organ for its manifestation nor upon a fixed mode of response in any given organ but primarily involves adaptation to the external environment including the latter's social phases. Attention was also directed to the difficulties inherent in classifying psychological forms of behavior because of the character of the behavior studied. These difficulties are not present in physiology since the latter science studies behavior which can be classified in terms of the structures involved.

The facilities for the meeting were uniformly excellent as regards dormitories, dining arrangements, and size and location of the rooms and auditoriums used for the various programs. Professor Bott and his associates proved themselves to be ideal hosts.

University of Minnesota DONALD G. PATERSON

THE NATIONAL RESEARCH COUNCIL'S FUND TO SUPPORT RESEARCH

The National Research Council has been given the administration of a limited fund from which grants can be made in comparatively small amounts toward the support of the research of American investigators in the natural sciences. The field includes Psychology.

The Committee in charge of the Fund has adopted the following administrative policy:

(1) Grants will be made to cover such expenses as apparatus, materials and supplies, technical assistance, and, in special cases, field expenses.

(2) In general, grants will not be given for personal salaries or fellowship stipends, for expenses of publication, for the purchase of books, or for travel to attend scientific meetings.

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K. M. Dallenbach	Studies in neural regeneration
E. C. Tolman	The inheritance of maze-learning ability in rats
F. Fearing	The functions of the non-acoustic portion of the labyrinth in pigeons
J. A. McGeoch	The influence of the time-interval and of the point of interpolation upon degree of retroactive inhibition
Sophie De Aberle	Growth and development of southwestern Indian children

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M.B.

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M. B.

A CORRECTION

In my note on a "Neglected Possibility in Frequency Theories of Hearing" in the October 1931 number, the figure for the refractory period, on the basis of which the calculations are made (pp. 660 f.), should read 0.005, not 0.02 sec.

University of Berlin GEORGE KREEZER

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Social Psychology: An Analysis of Social Behavior. By KIMBALL YOUNG. New York, Alfred A. Knopf, 1930. Pp. xii, 674.

Starting with the definition "Social Psychology deals with the study of personality as it develops in relation to social environment," the author divides his subject into five parts as follows: (1) on the social setting of human behavior, including also a brief introductory chapter and also a discussion of group life of animals and men and the nature of culture; (2) on the psychology of individual behavior, differing only slightly from the usual general introductory psychology; (3) on personality and group participation, covering the topics of language and social interaction, early conditioning, and influence on the personality of primary and secondary group contacts, occupational attitudes, leadership, authority and prestige; (4) on personality and subjective patterns as symbolized in language and exhibited in group life especially in myths and legends, and also on various types of race, national, religious and occupational personalities and prejudices; and (5) on the crowd and the public as groups in relation to personality, ending with a discussion of the psychology of the audience. There is an index of names and of subjects. At the close of each chapter suggestions and references are given for the student. The book is planned to be used as a text in connection with the *Source Book for Social Psychology* by the same author.

The style is clear, and interest is insured by the use of many concrete incidents and facts. It is up-to-date, and as objectively scientific, and nearly as well organized as the present state of the science makes possible. It will undoubtedly take a high place among works in this field, and be very useful as a text. For the latter purpose Part Two could easily be omitted for students who have already had courses in psychology. It would probably have been better to omit technical details, especially physiological psychology, and to make this portion a briefer introduction to the data of general and individual psychology, necessary to the understanding of social psychology proper.

It is always difficult in organizing a new body of scientific knowledge to determine the chief center of interest and fix the bounds of the science. It was an important change in the development of the science of social psychology when Allport demolished the indefinite, unfruitful idea of a "group mind" and centered interest on the individual, but in the opinion of the reviewer, another shift is needed. If we transpose Young's definition thus: Social Psychology deals with social environment as it develops in relation to personalities—we have a change in the center of interest that will be useful in the future development of the science. Ultimately it will be necessary to recognize that the center of interest for social psychology is neither personality as such, nor social environment as such; but these two factors interacting under certain physical conditions in determining group conduct and in developing human institutions. The latter aspect of social

psychology is not ignored by the author, but the development of individuals rather than of institutions is stressed, although perhaps not more than is justified in the present state of knowledge of the social sciences.

Leominster, Massachusetts E. A. KIRKPATRICK

The Language Development of the Preschool Child. By DOROTHEA A. MCCARTHY. Minneapolis, The University of Minnesota Press, 1930. Pp. xiii, 174.

This monograph begins with an historical sketch of the main contributions in this line by previous investigators. The study was carried out on a total of 140 children, 20 divided evenly as to sex representing each of the following ages: 18, 24, 30, 36, 42, 48 and 54 months. The observation on each child was made within a month and a half of its classified age, and the subjects were so selected as to be fair samples of the preschool population of Minneapolis. For this selection the occupation of the father was used as criterion, and the Barr-Taussig Six-Fold Classification of Occupations was applied to the adult male population of Minneapolis between the ages of twenty-one and thirty-five years, the percentage of each type of population being calculated from the 1920 census report. The mental test records of the children, by the Kuhlmann Revision of the Binet Scale, showed the mean I. Q. to be slightly above 100 at each age-level represented, and the girls were slightly superior to the boys. Despite very careful procedure it was, however, not possible exactly to equalize throughout all groups such factors as the coöperation of parents, the interest of parents in the free clinics and the greater stability of the population from which the samplings were chosen. Each child was observed individually in familiar environment for a period during which fifty consecutive verbal responses were recorded under carefully controlled conditions. Any response marked off from the preceding and succeeding remarks by a pause was considered as a separate unit.

An analysis of the results reveals certain recognizable stages of linguistic development and changes in the relative proportion of the different "parts of speech." It is observed that words as used by the child hardly fit into the different parts of speech as outlined by grammarians. Often a word may represent a complete sentence, and its classification by adults is difficult. In early stages of development the number of incomprehensible responses was large, and with advance of age it decreased faster in the case of girls than in that of the boys. The same order of development was also found through changes from simple to complex sentences, showing, in conformity with the results of intelligence tests, greater precocity in the girls. The details of changes in emphasis on different parts of speech as age advanced were not significantly different from those already well known. Though the psychologist's contributions to the knowledge of language development has in the past been small, it is evident that his part in the future scientific studies of this kind is likely to be considerable. The main contribution of the present study seems to be the greater precautions as to representative samplings, larger number of subjects and impartial recording of linguistic responses. It is obvious that much is yet to be done in recording objectively these responses through a long period of time. The children of the upper classes in general showed more rapid development than those from lower social strata. It is interesting to note that the child who is barely making a beginning in language

at 18 months gains proficiency in his vernacular, learning several thousand words for use in various more or less complex sentence relationships, in the short period of three years. J. P.

Statistical Résumé of Spearman's Two Factor Theory. By KARL J. HOLZINGER. Chicago, University of Chicago Press, 1930. Pp. 43.

In this forty-three page monograph, Holzinger has given a more clear and readable presentation of the statistical aspects of Spearman's two-factor theory than has appeared in any one publication.

The résumé includes the derivation of the tetrad difference function, an account of the probable error of the tetrad difference, and also the best approximation formulas for the probable errors. A list of all the possible tetrad differences of five variables and their probable errors is given along with a sample problem showing how these formulae are used.

Finally, there is a discussion of other factor patterns. Such patterns, while of theoretical importance perhaps, do not seem to fit most of the empirical data any better than the simpler two factor pattern of Spearman. In addition, the statistical procedures necessary to an investigation of group factors are much longer and more clumsy to handle.

If one wishes to make use of the technique of the tetrad difference, this monograph will prove very valuable.

Ohio State University HAROLD A. EDGERTON

Pleasure and Instinct: A Study of the Psychology of Human Action. By A. H. BURLTON ALLEN. New York, Harcourt, Brace & Co., 1930. Pp. ix, 336.

Following a brief sketch of theories of pleasure and 'unpleasure,' or feeling, regarded both as affective and as akin to sensations, the author takes the position that "Pleasure is the mental element which accompanies the sensations of the bodily processes when they proceed in a normal fashion" (p. 60). With strong stimulation pleasure may be enhanced. Unpleasure occurs with conflicts among the vital impulses as affected either by under- or over-stimulation. But what does this have to do with the control of human action? The reader who asks this question critically with an idea of getting an insight into the methods of control of the vital processes finds nothing of a scientific nature, but only unverified assumptions or implications of causation by subjective phenomena which are associated with activity (probably only different aspects of it). The results are not markedly different from attempts to explain a man's life activities from the movements of his shadows. 'Conation' plays an important part in the general speculations of this volume. Many of the British psychologists and philosophers would seemingly profit from a thorough and critical analysis of the implications of their concept of conation which plays no part in American psychology.

A discussion of some of "the human instincts," largely according to McDougall's early conception, including the 'impulse to power,' altruism (also treated as an instinct), and others, as they are related to conation and the general struggle to maintain the basic nutritive processes, occupies over 130 pages. The inheritance of acquired tendencies is assumed without regard to current biological

data to the contrary. Self-consciousness is regarded as playing a part in a rudimentary form in such 'instincts' especially as fear and anger, and in the higher 'conative' developments they emerge as the more explicit consciousness of the unity of personality. But even this higher stage, fortunately, is found to come into being as a result of our mental constitution and not by direct 'individual choice.' Later chapters deal with relations of feeling to instincts and to sensations, aesthetic experiences, ideomotor action, and 'the psychology of values.' No positive contribution to psychology is made in the book, which follows the older arm-chair, reflective methods with not a touch of scientific procedure. The volume is of little value to the individual who is attempting by verifiable hypotheses and experiments either to extend our knowledge of psychology or to learn about its established results. It is indeed unfortunate that books of this kind apparently consume so much of the study-time of serious-minded students. There are, of course, points of view from which the book is interesting.

J. P.

Encyclopaedia of the Social Sciences, Vol. I. EDWIN R. A. SELIGMAN, editor-in-chief; ALVIN JOHNSON, associate editor. New York, The Macmillan Co., 1930. Pp. xxvii, 646.

For the psychologist, the appearance of the first volume of the *Encyclopaedia* raises the interesting question of the relation between psychology and the so-called social sciences. The editors designate psychology as one of the semi-social sciences, along with ethics, education, and philosophy; the social sciences proper are politics, economics, law, anthropology, sociology, penology, and social work. In the case of the semi-social sciences those topics are selected of which the social aspects are acquiring increasing significance (preface, p. xx).

"Psychology is . . . becoming social in a double sense. We must interpret the individual mechanism of mental processes, in part at least, in terms of a social environment; and secondly we have to deal with the thought processes of the group as such, that is, of the individual not simply as unconsciously influenced by others but as purposively coöperating with others" (Introduction, What are the Social Sciences? Seligman, p. 7).

One can always quarrel with classifications. It would seem rather arbitrary to regard education as only semi-social when anthropology, with a large content on the physical side allying it closely with the biological sciences, is designated as one of the social sciences properly so called. As for psychology, the decision as to whether it should be more properly allied with the social or with the biological sciences must wait upon the determination of the extent to which individual mental mechanisms are the product of the social environment. In that connection Seligman seems to have chosen his words very happily when he speaks of psychology as "becoming" social. The extent of this "becoming" remains to be determined.

Rather more than half of this first volume is introductory. There is an historical account of "The Development of Social Thought and Institutions," sketched in broad outline from the time of the Greeks until the period of War and Reorientation; it is perhaps inevitable, in view of the range and compass of the outline, that much of it should be somewhat obscure except to the initiated.

The account of "The Social Sciences as Disciplines," which constitutes Part Two of the Introduction, describes the status, past and present, of the Social Sciences in the various European countries, Latin America, Japan, and the United States. Psychologists will undoubtedly wonder at the very rare and inadequate references to their science throughout Part Two. In the section on Great Britain are at least offered the explanation that psychology there has claimed affinity with the natural rather than with the social sciences; in the other sections, including that on the United States (L. L. Bernard) psychology is all but ignored. This is all the more surprising in view of the statement in Part One that "no summary of the definable influences of psychology can give an adequate account of its real importance for the social sciences in the post-war period. Especially in the latter part of the period psychology was 'in the air.' No social scientist not altogether impervious to the general movement of thought could fail to pay it tribute in the laying out of his problem, in his critical judgment of his own methods" (Section on War and Reorientation, p. 197, written by Editorial Staff). The writers of Part Two evidently did not make quite the same appraisal.

In the *Encyclopaedia* proper (Aaronson-Allegiance) there are a number of articles of definitely psychological interest. Jastrow writes on abnormal psychology and gives an impartial presentation of a wide range of topics,—hysteria, neurasthenia, Freudianism, extraversion-introversion, Kretschmer's types, endocrines, the autonomic nervous system, intelligence testing, maladjustment in children, crime, etc. One may question the possibility of giving any adequate idea of Kretschmer in one paragraph, or of Freud in three; certainly it would not be easy to understand these paragraphs without a background in the subject, but limitations of space perhaps made that inevitable. More surprising, in view of the nature of the *Encyclopaedia*, is the inadequate treatment of the social implications and applications of our knowledge of abnormal mechanisms, for example in certain religious manifestations, in mob phenomena, etc.

Miriam van Waters writes a good review of the topic of adolescence; L. S. Lyon treats advertising with hardly a word about the contributions of experimental psychology; Raymond Pearl discusses alcohol in its biological aspects, and comes to the significant conclusion that its moderate use is not harmful, and possibly beneficial; W. A. White defines alienist, and Kimball Young discusses adjustment.

In connection with the individual articles, the probability is that the psychologist who reads wisely will find the *Encyclopaedia* useful and interesting because of what it can tell him about the other social sciences, rather than about his own. He will hardly learn much from short articles, however well written, on topics in his own field. On the other hand when he reads—to take only one example—in the articles by Lowie on primitive adoption, of some of the interesting modifications and variations of what we have been accustomed to call the "parental instinct," he can get a hint from anthropology on the manner in which cultural factors affect some of our apparently fundamental psychological mechanisms. The function of the *Encyclopaedia* seems to be to make the knowledge of related sciences, rather than of our own, accessible to us. And in this connection it should be particularly valuable to psychologists, who have on the whole paid less attention to the other social sciences than they have paid to psychology.

On the Editorial Staff, representing psychology, are Floyd H. Allport, who is Advisory Editor; Georgina S. Gates and Mark A. May for the American Psychological Association; Edward L. Thorndike for the National Education Association. Margaret F. Washburn is on the Board of Directors, and a number of other psychologists act as Editorial Consultants.

The *Encyclopaedia* as a whole represents a notable effort at coördination, which promises, in spite of certain inevitable difficulties, to be a highly significant contribution to the development of the social sciences. The remaining volumes (fifteen in all are projected) will be eagerly awaited.

Columbia University — OTTO KLINEBERG

Direction Orientation in Maze Running by the White Rat. By JOHN F. DASHIELL. Comparative Psychology Monographs, Vol. 7, No. 2. Baltimore, The Johns Hopkins Press, 1930. Pp. 71.

This monograph is an experimental study of the commonly observed tendency of the white rat to exhibit a general direction-orientation in maze running usually toward the exit and more or less independent of specific and local stimulation. In preliminary experiments this showed itself as a tendency to make errors in blind alleys that happened to open up in the general direction of the food box much more than in those that opened up in the opposite direction, regardless of the direction of the true pathway at such points—a finding which agrees with the earlier results. The problem of determining the nature of this reaction is studied extensively in the present monograph. Exteroceptive sources of guidance (visual, auditory, and olfactory cues both from within and without the maze) were eliminated by the usual methods of rotating the maze, changing the materials used in the construction of the maze, and using air currents from false food boxes, the experimenter, etc. By adopting specific modes of inserting animals it was found that animals tend to follow out their initial orientation to a much greater extent than could be accounted for on a chance basis. On the theoretical side, the author suggests that the rat on entering the maze takes on some sort of kinesthetic or organic posturing set developed from previous trials and directly related to the animal's position when proceeding up the entrance passage-way. This general factor persists and reasserts itself when the animal temporarily turns aside from the true pathway, and thus operates to facilitate movements consonant with this orientation and to inhibit antagonistic responses. This explanation may perhaps be regarded as a special application of the 'completeness of response' principle of Peterson to the particular conditions investigated.

Columbia University — C. J. WARDEN

The Experimental Production of Dreams during Hypnosis. By DAVID BALLIN KLEIN. Austin, University of Texas Press, 1930. Pp. 71.

"I want you to watch very carefully for any dreams you might have and as soon as the dream is over tell me all about it." With these instructions to (mainly) eight hypnotized subjects and with a 10-second stimulus of various kinds, including a pinch on the hand, a whiff of perfume, sound of a tuning fork, handling a wax candle, strokes on the hand with absorbent cotton and near the genital region with a ruler, 171 hypnotic dreams were recorded—only 40 of which are printed in

this monograph. Rearranged and reworded, the author's main conclusions are: (1) Hypnotic dreams are essentially like sleeping dreams, and differ from associations that result from asking waking subjects to dream (a control run with three subjects) in being "real," "present," and in having a *Gestalt*-like pattern, to which the stimulus is assimilated and lost. (2) The average hypnotic dream—records for 84 dreams—ran for 32.5 seconds. (3) Sex dreams were rare, only 9% of the total; the Freudian distortions non-evident. (4) The existence of typical dreams in stereotyped situations, (*e.g.*, the dream of falling at tilting the bed, of smothering at covering the face) and of recurrent dreams by a given person with the same stimuli, was confirmed. (5) Dreamless sleep may be merely amnesic sleep. (6) The results bear out Hollingworth's idea of the redintegrative nature of thinking—the purpose underlying the series of experiments.

The method and the procedure are admirable and the results and conclusions are very suggestive. It occurs to the reviewer, however, that contrary to the author's findings, many hypnotic subjects do dream spontaneously, being *en voyage;* and that the very suggestion to dream and to report the dream as soon as it is over may constitute not only a fictitious setting, but also an artificial beginning and an arbitrary ending of the dream. The patterns of the hypnotic dreams may be smaller, of shorter duration, less complex and much more definite, and even qualitatively different because the antecedents in hypnosis—including the instructions—are different from the antecedents in sleep. Perhaps the very nature of the dream itself is changed by being on the alert for a dream, instead of being intent on sleeping. But even though it were shown that the results of these experiments could not be predicated of normal dreaming, they would still be interesting and revolutionary.

Louisiana State University PAUL C. YOUNG

The Child of Circumstance. By ALBERT WILSON. New York, William Wood & Co., 1929. Pp. xx, 420.

This book represents, the reader is blandly told, a surgeon's matured reflections on crime and his scientific determination of its organic and structural concomitants. It is difficult to treat the elaborate pretensions of this work seriously, for everywhere the psychological naïveté of the writer is painfully manifest. Such a work may be ascribed to the curious and pathetic belief, held less strongly at present, that medical training qualifies one for pronouncement on anything, especially if the thing could be dubbed 'scientific.' The reader is dismayed by the enviable ease with which 'The Psychology of the Criminal' is disposed of in two pages; the equally facile classification into 'types;' and finally the assured reduction of all criminal behavior to defects in the structure of the brain.

As criminology, psychology, and biology the claims of this book are very dubious, indeed. But as a significant document on the aberrations of the scientific mind it may have some value perhaps, for it shows that a scientific technician, like a surgeon, may easily live in two hermetically sealed worlds: one in which all the safeguards of scientific procedure are enforced, and another in which only the terms, the symbolic forms, of science are present without meaning.

University of Louisville ELLIS FREEMAN

Teaching in College and University: A Survey of Problems and Literature in Higher Education. By C. V. WOOD. Baltimore, Warwick & York, Inc., 1929. Pp. xi, 557.

This book, as the sub-title indicates, is a general and necessarily superficial survey of the problems and literature in higher education; it also records mere opinions, several of which are unimportant and are given by individuals who themselves have done no experimental or other important work on the problems. This is probably a necessity for the sake of completeness of such a survey, and because problems and opinions are not easily differentiated. The two chapters dealing with the psychology of learning and with measurement and guidance touch most directly the work of psychologists but they omit many valuable contributions and contribute nothing specifically. It is true, of course, that several psychologists are now doing administrative work of importance. The book should be valuable to all workers in higher educational institutions, chiefly because of its immense classified bibliographies, those in the Appendix alone covering 83 pages; and yet the reviewer finds the bibliographies of the decade covered (1918-1927) very deficient so far as the more serious experimental investigations are concerned. Much useful psychological material and important scientific techniques have been omitted. It is probably such experimental procedures which ultimately will be invoked to settle most of the problems considered. The professional educator has yet a long way to go to bring his problems under experimental control and handling.

J. P.

Methods of Private Religious Living. By HENRY NELSON WIEMAN. New York, Macmillan Co., 1929. Pp. 219.

The chapters of this book first appeared under the title of "Experiments in Personal Religion" during the year 1927-8 in the monthly bulletin of The Institute of Sacred Literature published at the University of Chicago. The book lends itself to classification as a phase of applied psychology. It constitutes an attempt to apply certain methods to a definite sort of human activity.

The theme of the book as a whole is that of progressive integration. There are nine chapters, eight of which are devoted each to a particular method, while one, the ninth, considers several methods. These methods are offered as guides "in personal experimental ventures of religious living." Of the various activities employed in making these methods effective, the following are illustrative: social adaptation and organic association; relaxation; self-analysis; auto-suggestion; the acquisition, evaluation, and organization of facts; developing habits of alertness, plasticity, and fearlessness; developing habits of experiencing beauty; holding fixedly before the mind some belief or ideal until it exercises "a powerful influence upon the individual;" and problem-solving by "exposing oneself to the stimulus of a problematical situation with a mind freed of all bias and preconception, and waiting in this state, or returning periodically to it, until there dawns upon the mind that integration which will solve the problem."

The book is excellently organized, and should be of interest to psychologists as indicating an effort to apply psychological principles, by one who is not primarily a psychologist, to a definite form of human activity.

Syracuse University ERNST THELIN

Psychologische Untersuchungen über das Stiefmutterproblem: Die Konfliktmöglichkeiten in der Stiefmutterfamilie und ihre Bedeuting für die Verwahrlösung des Stiefkindes. By HANNA KÜHN. Hamburger Untersuchungen zur Jugend- und Sozialpsychologie, No. 1. Leipzig, J. A. Barth, 1929. Pp. viii, 162.

A new series of which this is the initial study takes for its field the important facts connected with the personality of the adolescent and his behavior in social situations. The present investigation belongs, both in its conception of problem and its method of investigation, to the last decade of the nineteenth century. The effort is made to isolate the single factor, the presence of a stepmother in a home, as a source of conflicts, and to determine the significance of such conflicts on the personality and the behavior of the stepchild. The answer to this two-fold relationship is sought in the replies of children to questions, in compositions on the general theme under consideration, in rather complete documents from a juvenile court, in biographies, literary references, personal interviews with children, and the like. In each case the reader is given only quotations to support an interpretation.

The scientific world has outgrown the method here used as a source of dependable knowledge. The study makes its most important contributions when it lifts into relief problems that require a precise formulation and an objective method for their solution and this careful survey of what people think and believe abounds in such problems. The extent to which popular opinion regarding stepmothers affects the behavior of every member of the new family group and the growth of personality under conflict situations are examples of such problems.

Florida State College for Women P. F. FINNER

The Neuroses. By ISRAEL S. WECHSLER. Philadelphia, W. B. Saunders Co., 1929. Pp. 330.

This book was written primarily for the medical student and the practitioner. It is of considerable value, however, to the psychologist whose interests are in this specific field of abnormal psychology.

The author discusses the various schools, but he always has a psychoanalytic leaning. In fact, Freudian designates his point of view better than psychoanalytic. He says that more than 90% of what constitutes genuine psychoanalysis was evolved by the originator himself.

In the chapter on etiology, which is one of the most interesting, we get the author's methods as well as his leaning. He says: "It is true that psychoanalysis furnishes at present the best insight into etiology of the neuroses, but there are a number of other theories, each of which has more than a grain of truth" (p. 89). He then proceeds to discuss and, to some extent, to evaluate the following theories: the somatic or pathological, the physiological and the behavioristic theory, the glands of internal secretion theory, the vegetative nervous system theory, the suggestion, the social or biologic, and the psychoanalytic theory.

The book is well written, interesting, and stimulating, but it is likely to be of little value to the person not well grounded in the various fields of psychology. The different theories described are not criticised and evaluated sufficiently, and the book assumes too much of the nature of an encyclopedia.

University of New Mexico B. F. HAUGHT

An Anthology of Recent Philosophy: Selections for Beginners from the Writings of the Greatest 20th Century Philosophers. By D. S. ROBINSON. New York, Thomas Y. Crowell Co., 1929. Pp. xii, 674.

The author has attempted in this volume to bring together for the beginner in philosophy—and also for non-professionals who wish to know something of what philosophers are saying—a representative selection of "the most readable" recent philosophical literature. The reader who takes the term 'anthology' too strictly will soon become aware that he is in for a serious piece of work, but he will doubtless find it interesting if he is naturally inclined toward philosophizing. First he is given on a single page a conspectus of the various "isms" and other types of modern philosophies—idealisms, realisms, pragmatisms, mechanisms, vitalisms,—a veritable "flower gathering" of the products of 'emergent' philosophers. It is freely admitted by the author that because of this selection according to types many worthy philosophers not exactly fitting these types are omitted. It appears reasonable, once you turn your attention to these 'emergents,' that in philosophy as in psychology (witness *Psychologies of 1925*, and our recent 'philosophical' discussions filling hundreds of pages in modern psychological books and journals) only the more distinguishing 'types' shall receive attention and analysis. A great service has been done for the reader by Robinson. Preceding every excerpt is found an excellent short and clear analysis or résumé of the selection, and after the latter are a number of questions which seem to be, in general, rather good to bring out a thoughtful consideration of the contributions of the selection as well as the point of view and the classification of the author (philosopher) who has contributed it. In this latter task the reader is helped by the first selection in the book, James' interesting and suggestive Philosophy and Temperament, *Pragmatism*, pp. 1-13 and 33-40. The author of the *Anthology* has made a real contribution in these features of a preceding analysis and really helpful questions, and the reader is asked to do his part in turn by reading each analysis and selection thrice,—first simply for general interest; secondly, to correlate analysis with excerpt and to try his hand at writing out a good analysis himself; and, thirdly, "to find out the answers to the questions at the end and to form his own opinions of what the author says." This last reading will, of course, help the reader also to see that any author's questions are more or less from his own point of view, total independence from such a more or less personal or 'school' slant being impossible to any author (especially the author of any 'philosophy').

Part I gives a general orientation; Part II exhibits the idealisms or "tender-minded" philosophies, to use James' characterization, and they do ring somewhat true to this characterization in the view of a humble layman like the reviewer; Part III presents, in the framework for tough-minded analysis already described, the realisms; and in Part IV we find the pragmatisms. It is well that the tender-minded get the stage first, for most of the later "isms" are exhibitions of more or less "tough-mindedness." To this statement, however, some exceptions must be made, for the reader will find in Part V, which is too recent, and perhaps too 'terrible,' to be characterized in a single term (here, by the way, is work for some future 'emergent'), along with the wicked and surely tough-minded "mechanisms," such selections as Emergent Evolution by Driesch, The Unknowable by Spencer, Fictionalism from *The Philosophy of "As If"* by Vaihinger, and the so-called New Scholasticism and Its Contributions to Modern thought, by Ryan.

The reviewer has added this hyphenated word "so-called," because he has found in the book nothing particularly new except that it appears in this 20th century, but he may not have delved deeply enough, not being a philosopher.

An additional valuable feature of the book is an appendix of biographical and bibliographical sketches of the philosophers from whose writings excerpts have been taken. The author seems to have done a creditable piece of work, and his *Anthology* will doubtless appeal to many readers other than beginning students in philosophy. Many scientists will doubtless find recreation in going over its pages.

J. P.

Fundamentals of Educational Psychology. By I. M. GAST and H. C. SKINNER. New York, Benj. H. Sanborn & Co., 1929. Pp. xiii, 354.

The material of this text is presented under the conventional chapter headings and every chapter is organized under a number of sub-headings. Every sub-topic is in most cases a unit in itself, and often there is little attempt to make a transition from one sub-topic to another. This gives the impression that the book is a series of disjointed sub-topics rather than a collection of developed chapters.

The authors do not hesitate to include material which bears upon a given chapter but which is more fully treated elsewhere. Thus, the first chapter includes not only a description of original nature but also a discussion of methods of modifying instinctive tendencies and a treatment of imitative and volitional action. Material belonging primarily to the chapter on transfer of training is presented in the chapter on habit. "Problem of attention" appears in the chapter on the learning process. Material on instincts and individual differences is introduced into many chapters. This breaking down of the logical barriers between chapters is perhaps advantageous from a pedogogical standpoint. On the other hand, the overlapping between the chapters on habit, the learning process, and memory seems to the reviewer hardly justifiable.

The book cannot be classified as to its central point of view. This is as the authors intended, for one of their aims has been to place "greater emphasis upon the significant contributions of the many psychologists regardless of viewpoint and less emphasis upon particular viewpoints." It seems safe to say, however, that they regard the individual as a product of 'will' as well as of heredity and environment. Although they assert that heredity and environment cannot operate independently, they place the emphasis decidedly upon heredity. Instincts are treated as drives, and the assertion is made that "they serve as a basis for all education." The treatment of learning follows that of Thorndike rather closely.

Another of the authors' aims has been to give "a frank expression of sane opinion concerning certain topics that have been misinterpreted or overemphasized in certain quarters during recent years, *e.g.* intelligence tests, the significance of test results, new-type classroom tests, endocrinology, heredity, and behavioristic psychology." The chapters on intelligence and educational measurements, which appeal to the reviewer as the strongest, fulfill this aim very well for the first three of the above examples but why the last three should have been mentioned is not clear. Heredity receives greater emphasis than is customary even in texts on educational psychology and endocrinology and behavioristic psychology are scarcely mentioned.

The outstanding feature of the book is the special attention given to the social and applied aspects of the subject. This has led to a great deal of generalization, quotation of opinion, and moralization. The result is that any psychologist will find much with which to disagree, the student will perhaps wish for greater concreteness in many of the applications, and if he is not inclined to be credulous, he may desire more support for many conclusions. Nevertheless the authors have, in the reviewer's opinion, taken a long step in the right direction in some respects and have achieved a practicality greater than that which is characteristic of many other texts in the field.

Stephens College CARL N. REXROAD

The Intelligible World. By WILBUR MARSHALL URBAN. New York, Macmillan Co., 1929. Pp. 471.

The author develops the theme that philosophy is intelligent discourse about the world and that the only intelligible language is the idiom of the Great Tradition. The secret of philosophical wisdom is in the intellectual orientation toward the fine minds of all time. Any concept of totality that shall satisfy the demands of philosophy must possess meaning and value, as well as reality. In fact every problem of metaphysics resolves itself into one of value. The only tolerable conception is of the world in the form of "idealism," that is, spiritual content is the fundamental reality. The traditional conception of time and space pictures them as phenomena of a deeper non-temporal, non-spatial order of meanings and values. "The One Spirit's plastic stress is the only element of unity or universality that can be found in the world process. Evolution *makes the world intelligible to us only* (italics mine) when we conceive it, either consciously or unconsciously, as evolution of a divine idea, as a gradual revelation in time of that which was present from the 'foundation of the world.' "

Teleology, finality, and purpose, properly understood are a necessary part of the world. Evolution and progress are in truth transcendental or metaphysical, or they are nothing. A decrease in the amount of available energy in the world entropy the author interprets as a problem not of metaphysical but of cosmic order, being nothing more than the cosmic application of the significance of death. Entropy, he contends, does not hold true for life and mind. Immortality has always been a symbol for the conservation of value. It is in this sense only that endless life has been a necessary postulate of morals and values. The only world possible to intelligent reflection has as its constituent parts Matter, Life, Mind, and Spirit.

Salient conclusions from the speculations afford comfort to those in sympathy with the dualistic point of view in psychology. The arguments depend considerably on experience for their validity, though the point of view is not scientific. Noteworthy spiritualizations of experiences are the timeless-present, 'instinct of philosophy,' immanence, and omnipresence. The book should appeal to the vitalist as a scholarly return to the idealism of the past in the light of the present. In contrast, to the modern critic it should appear, perhaps, as a learned examination and organization of many types of human conduct, making value stand out as one of the great present day problems.

University of Utah M. C. BARLOW

The Art of Interrogation. By E. R. HAMILTON (with an introduction by C. Spearman). International Library of Psychology and Philosophy and Scientific Method. New York, Harcourt, Brace & Co., 1929. Pp. xii, 174.

As the title of the book suggests, interrogation, in the form of tests of intelligence or of subject matter, is an art as well as a science, though a very young and imperfect science. The author has selected for discussion three general topics: namely, mental tests, examinations of the old style and new, and the questioning conducted by the classroom teacher.

The book is quite free from technical terms and altogether free of statistical devices, being intended, evidently, for the non-technical reader; especially, it appears, for the teacher. It is the author's purpose to consider the basic psychological principles of interpretation of tests; for, in his own words, "though examinations are of such long standing and intimately affect so many people, a critical survey of the principles governing their use has but seldom been attempted." The author develops his thesis chiefly by means of presenting some of the main principles of test construction, such as the determination and control of selected mental processes, selection of representative items, correlation of processes, consistency of conditions, quantification of results, and so on.

In connection with the remaining two devices—old and new type examinations and questioning—it is the author's thesis that examining is an art, having at the same time "a scientific basis which is imperfectly understood." He expects psychological research to make this understanding less imperfect as more is revealed concerning learning and expression, knowledge and ability, examiner and examinee.

The non-technical reader will find in this volume a clear statement of some of the important psychological principles underlying various types of testing, and teachers of all grades will find suggestive principles and criticisms of interrogation, both oral and written.

Cornell University F. S. FREEMAN

Cornell Studies in English XVI, Gianfrancesco Pico della Mirandola: On the Imagination. The Latin text with an introduction, an English translation and notes. By HARRY CAPLAN. New Haven, Yale University Press, 1930. Pp. 102.

Pico's work on the imagination is an essay on a psychological subject, which is here fittingly presented by a student of the classics in a series of English studies. This combination of interests matches the complex nature of the original work. Pico was a humanist, and the humanist of his period (1470-1533 A.D.) was a man interested in the classical tradition, given to scholarly contemplation, and not so far penetrated by the new sciences as to ignore the work of 'supernatural agencies.' All these factors appear in this little work.

In the history of psychological literature imagination or phantasy occupies an important place. It mediates between sense and reason, for its material is given by the senses, but it is itself an inner activity. As the images survive the moment of sensation they become objects of reflection, partly dominating the acts of reason and partly used by reason to embody its more abstract visions. Desire is allied with imagination, and for that reason the subject leads into questions of conduct. The physiological part is supplied by the ancient doctrine of tempera-

ments, against which the reason sets the control of ideals in the form of imagined good or evil. Pico is obviously humanistic in the sense that he aims to make his psychology the basis of a practical ethics.

The work naturally appeals more to the historian of ideas than to the modernist, who cares only for the latest fruits of science. Pico thinks the variety of imaginations depends on the temperament of the body, the sense affections, the judgment and the ministration of the good and bad angels. The last source effectively dates the work. Pico thinks disorders of the body must be cured by treatment of the body. The others he would treat by what we may call forms of sublimation: like some modern psychologists, he relies on religious emotion to achieve the most effective integration of life. His views on the use of heaven and hell, that children may be "delighted by the one and saddened and terrified by the other," are tempered with a flash of the experimental method when he says: "I have discovered by experience with my son that stories of this nature have had this effect," and he anticipated Bacon's views that in education concrete pictures have more power than abstract reasoning. Ignoring the severe standards of modern science, let us acknowledge the urbanity and charm of Pico's essay. The work of the translator has been well done. The notes are valuable and show competent knowledge of the sources and the subtle transitions of mediaeval thought from the purely classical to the mixed Neo-platonic, Arabic and Christian teaching. The printing is exceptionally accurate and the whole production does credit to the industry and scholarship of the author.

University of Toronto G. S. Brett

Twins: Heredity and Environment. By Nathaniel D. M. Hirsch. Cambridge, Harvard University Press, 1930. Pp. 159.

The author has written a treatise on twins which will interest both the scientist and the layman. There are brought together in the first chapters the salient biological and psychological facts concerning twins. A chapter is devoted to a review of most of the important experimental studies upon twins. With this introduction Hirsch launches into a 'fresh' attack, which, far from yielding simple descriptive data, is to disentangle the effects of heredity and environment. The method is simply that of experimental selection, surely nothing new and a procedure to be followed with the greatest caution.

Data are gathered upon dissimilar twins living in a similar environment (58 pairs), similar twins living in a similar environment (38 pairs), and similar twins living apart (12 pairs). Obviously criteria of similarity and dissimilarity were required. For the twins, these criteria were provided by physical and mental measures. These seem quite adequate. For the environments, similarity was thought to obtain when the twins lived under the same roof. This selection is questionable.

Each pair, having been selected, is described with respect to certain anthropometric measures (height, weight, cephalic index, etc.) and mental measures. In addition there are often given data concerning handedness and other traits. Much of the descriptive material is subjective and much is drawn from the estimates of school teachers and others. It is doubtful whether this material is to be trusted. One is impressed either with the similarity or the dissimilarity of twins.

In the former case there is doubtless an unintentional stress, on the part of teachers, social workers, and friends, on likenesses; in the latter instance contrasts are readily drawn though differences may not actually be very great. The subjective estimates of character, temperament, and emotionality are thus driven to extreme statements. However, this kind of material is not of such a nature as to make general conclusions possible. The author has concerned himself therefore with those traits capable of quantitative expression and interpretation. After summing up the measures certain contrasts are made. Comparisons, showing little statistical reliability, yield the sweeping conclusion that heredity is several times as important as environment in determining the extent of the differences found between members of a twin pair. Obviously the yield is a function of the selection process and this will be found upon examination to be not above criticism.

In the last chapter the author makes an impassioned plea for change in the social order.

University of Virginia FRANK A. GELDARD

King Mob: A Study of the Present-Day Mind. By FRANK K. NOTCH (pseudonym). New York, Harcourt, Brace & Co., 1930. Pp. 226.

This volume, like a telegram, is an urgent, brief communication. Much must be read between the lines. The author recognizes a consuming struggle between individuality and collectivity, between nature and spirit; individuality and spirit being the things consumed. He fears that nature and its counterpart, collectivity, are reducing culture to a meaningless mechanism. The holocaust now impending has its historical precedents. But, in consequence of modern instruments of communication and the perfected mechanics of exploitation, the present-day mind represents a complete surrender to the Moloch of "a thrilling awareness of number."

One lays down this important book with a loss of respect for democracy: the voice of the people is not the voice of God, but the meaningless urgings of some as yet unknown "elementary law of nature." It might reasonably be concluded that this elementary law of nature is an interpretative rather than an explanatory principle: "The enmity between the Mob and the individual thinker . . . [is] a war in which the weapons of the moment are fortuitous, but in which the opposing energies flow from the inexplicable dualism of the world" (p. 13).

In the light of this general principle the author surveys the American mind in respect to literature, science, commerce, and national and international politics. And there he finds nothing but a reservoir of stupidity, ignorance, and self-deception. With pardonable dignity he represents himself as a prophet. His inferences are drawn from a wide and reflective observation of recent social tendencies. The reviewer surmises, however, that the author is not entirely unprejudiced in his attitude toward certain modern popularizers of our evolving cultural products.

The volume makes no pretense of being a contribution to social psychology as science. It does, however, represent a clever combination of the philosophies of Tarde, Le Bon, and Durkheim. Drawing upon them (perhaps unconsciously), the author portrays our contemporary America as a product of universal repetition, collective stupidity, and group coercion. His style is lucid, his reasoning generally consistent. The reviewer is, however, unconvinced of the validity of his premises.

May it not be that the alleged universal war on individuality by the mob is something other than the author finds, perhaps the very opposite of it? May not this collective 'taking to the new' really mean a collective search for individuality? Those who refuse to be drawn into intellectual stampedes about social movement generally will be inclined to answer these questions in the affirmative.

University of Maryland J. W. SPROWLS

The Vibratory Sense and Other Lectures. By DAVID KATZ. Orono, Maine University Press, 1930. Pp. 163.

These lectures, written in semi-popular form, are based mainly on the works of Katz and his students at the Rostock Psychological Institute. Experimental and observational studies, mostly with animal subjects, indicate that while the physiological state of the organism conditions hunger to some extent, hunger depends to a high degree on the external circumstances in the immediate environment.

Children are classed as "voluntary vegetarian" and "voluntary carnivorous" on the basis of expressed preferences for different kinds of food. However, since no history of the development of individual food habits appears to have been considered, this classification can have little meaning.

In studying facial characteristics a composite photograph of ten intelligent and of another of ten unintelligent children were made and compared in various ways. It is quite possible that this technique is not valid. The results obtained in various investigations on judging intelligence and personality traits from photographs suggest the possible hazards involved in an analysis from composite pictures of facial characteristics which accompany intellectual traits. The method is, of course, not new.

Although the author warns the reader against Darwinian anthropomorphism, he rather uncritically describes the character of hens in such terms as "she was very envious," and "she did not like to be the subject of examination," and even tends to approve of anthropomorphic interpretation of animal behavior.

Other topics considered are (1) localization of sound by dogs, cats, and hens; (2) the "vibratory sensation;" and (3) conversations of children.

The book as a whole contains much information and stimulating observation. Psychologists and others will find it an interesting and valuable treatise.

University of Minnesota MILES A. TINKER

Folkways in China. By LEWIS HODOUS. London, Arthur Probsthain, 1929. Pp. vi, 248.

This volume is a collection of material on folklore and customs gathered from personal observation by the author during long residence in China, from conversations with Chinese and other residents, and from "wide reading" in Chinese sources, *e.g.*, the *Book of Rites.* The thirty-seven short chapters cover a great variety of topics dealing with ceremonies and feasts, observances relating to celestial phenomena and seasonal changes, to agriculture, great alleged historical events, war, sacrifice, etc. There are no specific references and the reader is given no means of evaluating the data as to validity. The book, though interesting to students of human nature, is not scientific. General references and a list of Chinese names are given at the end of the book. J. P.

Great Experiments in Psychology. By HENRY E. GARRETT. New York, The Century Co., 1930. Pp. xvii, 337.

The book has been planned as a second text for the elementary course. It aims to supplement standard texts by presenting concretely the experimental foundations of psychology.

The "great experiments" include the work of Binet, Terman and other testers, the army alpha, Ebbinghaus' studies in memory, Pavlov's investigations upon the conditioned reflex, Thorndike's animal experiments and laws of learning, the work of Thorndike and Woodworth upon transfer of training and formal discipline, Watson's experiments with infants, Galton's studies of individual differences, Cattell's researches on reaction time, the experiments of Cannon and others upon emotion, Köhler's investigations in perception and learning, the laws of Weber and Fechner, studies in space perception, and the work of Franz and Lashley upon brain function during learning. Each field of experimentation is treated broadly and with a wide historical perspective. Earlier and later work is brought into the same picture with excellent balance and arrangement.

The treatment throughout is accurate and dependable. A great deal of material has been organized in a way which will inspire the alert and industrious student. Each chapter closes with a few carefully selected references. There are eleven plates portraying the psychologists whose work is considered in the text; and thirty-five figures.

This book should give the elementary student a sound and thorough introduction to psychology *via* experimentation, and leave him with a wholesome respect for the subject. The reviewer is delighted with the volume and hopes it will be widely used. In addition to other merits of the book, the style is lucid and very readable.

University of Illinois PAUL THOMAS YOUNG

University of Iowa Studies in Psychology, No. XIII. Ed. by C. A. RUCKMICK. Psychological Monographs, Vol. XL, No. 181. Princeton, Psychological Review Co., 1930. Pp. vi, 214.

Space does not permit giving both full abstracts and detailed criticism of each of the papers in this monograph; nor does the importance of most of the papers warrant any deviation from this rule. The reviewer will therefore limit himself to brief remarks, allocating them to their proper fields in psychology and attempting to rate their relative excellencies.

Harold R. Fossler. Disturbances in Breathing During Stuttering (pp. 1-32). The presentation of this paper is excellent both in clearness of style and ordering of the material. The importance of the paper consists chiefly in an improvement in technique. Previous experimenters in this field have caused their subjects to stutter by making loud sounds and bringing about other emotional disturbances, all of which adds irrelevant deflections in the graphic records. Fossler requests his subjects to talk freely about some past experience and takes his stuttering as it comes. The only criticism of the method is that these past experiences are sure to have different emotional contexts, which somewhat defeats the improvement in technique. Furthermore, no complete record of this free exposition is taken,

as could easily be arranged with a dictaphone. Even though the author finds that special words do not bring on stuttering, the breathing curves mean very little without some correlation with the *S*'s phraseology and ideas expressed.

William Severt Larson. Measurement of Musical Talent for the Prediction of Success in Instrumental Music (pp. 33-73). The abilities tested by the Seashore tests may well be necessary in varying degrees to performers whose instruments demand special acuity in certain fields as supplements to their instruments, but they are by no means criteria of musical talent. The latter lies more on the conceptual level, and does not demand anything exceptional in the way of acuity. A deaf Beethoven and a Clara Schumann distinctly hard of hearing can compose or perform superlatively, but many a piano tuner, with well-nigh perfect pitch, can do neither. This analysis makes understandable the fairly high correlations found by Larson and other experimenters in this field, for naturally a performer on a stringed instrument has to have a good ear or he will fail, no matter how great his endowment of musical talent may be. But more important still, it explains why even in the most proficient group tested by Larson there are students rated by the tests in the last two percentiles of all the groups, who apparently are doing satisfactory work in music. These students would be weeded out, if Larson's recommendations were followed, long before they had reached this stage. This would be manifestly unfair to them and it would perhaps render music a decided disservice.

J. T. Nielsen. A Study in the Seashore Motor-Rhythm Test (pp. 74-83). The paper is very unclear—no statement even of the rhythms used is made. The author finds that both musically trained and untrained *S*'s improve but slightly during the first three days in their ability to tap out a rhythm sounded in the ear. Considering that the music students improve somewhat more than the others in spite of their previous training, it may be inferred that this period is largely spent in getting used to the experimental situation and the problem set. All except one of the music students rate higher in adeptness than the others, so that the author feels that another criterion of musical talent has been found. The reviewer is not so confident.

Emily Patterson. A Qualitative and Quantitative Study of the Emotion of Surprise (pp. 85-108). The qualitative parts of the paper may prove of value, but the quantitative sections, since they are derived by a galvanometric technique, for which there is as yet no accepted explanation, must remain open to question. The procedure is doubtful in one particular, at any rate. The *E* leaves *O* alone in the experimental room to set the Wheatstone bridge and to read his own "normal resistance." But *O*'s emotional state during this period is usually far from normal, ranging in different *O*s from sleep to terror; and since the value thus obtained is apparently used in later computations, a large source of error may have entered with it.

Alfred R. Root. Pitch-Patterns and Tonal Movement in Speech (pp. 109-159). To the reviewer, this paper is by far the most important in the volume, for it is not only a contribution to phonetics, but should give psychologists in general much food for reflection, in that it compares the physical frequencies applied to the ear with the phenomenal experience, and finds many striking disparities.

The implications for phenomenal psychologies are wide. The method should surely be carried over into music psychology to see how many of the characteristic slides and glides found by Metfessel are actually perceived as such.

Arnold Henry Wagner. An Experimental Study in Control of the Vocal Vibrato (pp. 160-212). The paper should be addressed to singing teachers rather than to psychologists. Its aim is to discover whether 'artistic vocal vibrato' may be taught. It may be.

Cornell University R. S. Hill

Studies in the Organization of Character. By Hugh Hartshorne, Mark A. May and Frank K. Shuttleworth. New York, Macmillan Co., 1930. Pp. xvi, 503.

This is Part III of *Studies in the Nature of Character* by the Character Education Inquiry, Teachers College, Columbia University, in coöperation with The Institute of Social and Religious Research, and it concerns itself with many of the same traits, situations, and tests as the preceding parts. Additional tests of inner traits and attitudes are given and treated in the same careful, objective, scientific, statistical manner that characterized the earlier studies. In this volume, however, the object is not to study and measure traits and situations themselves, but to discover and measure their relations to each other in the structure of character. The authors remain committed to the idea that all their tests are of particular ideas, acts, and situations, and valuable chiefly because of variety of sampling. Their final selection of a battery of twenty-three tests as a basis for a total character score, with directions for administering and scoring, are not included in this volume, but are published separately by the Association Press, 347 Madison Avenue, New York City. The theoretical validity of the group of tests, as computed, is 0.95. Detailed tables, formulas, and bibliography are given in the appendices. The index is for this volume only.

The authors discuss the relation of social intelligence and social attitudes as indicated by tests and they seek to determine the sources of moral knowledge and the influence of concomitants such as age and social intelligence as well as of environmental influences, such as home, school, Sunday school, and clubs. They investigate the interrelation of these factors, and reach the conclusion that conduct is more a matter of group standards than of individual traits. The components of character as revealed by reputation scores, foresight tests, combined pen portrait tests, and the relations of these to actual functioning in work and play are considered.

In studying the nature and significance of integration, especially in relation to the honesty tests, the authors find that only about 40% of children show integration as measured by consistency in the various honesty situations tested, and these in only a slight degree.

In the opinion of the reviewer, the small amount of consistency revealed by the test is due in part to adult lack of consistency in applying standards, which is reflected in the scoring of acts as more or less moral. The authors find a close, though small, correlation of indices of consistency with the total honesty score. They do not, however, find evidence that children consciously generalize from particular situations, and integrate in accordance with ideal standards. The

claim of the authors that the integrations are specific may be allowed without accepting the view that children do not try to apply general standards to particular cases. They, as well as adults, try to do so, but are likely to succeed only in lines where group practices guide them in deciding what acts are to be regarded as consistent with the general ideal.

The chief conclusions regarding character education are that present methods of moral training may do more harm than good, when ideals and the factors influencing conduct in practical situations are contradictory; and that the principal difficulty in character education of children, is the conflict of the standard set with the factors in the situation which naturally control the child's action. If the child were continually a member of a group observing standards of general social usefulness, the problem of education would be greatly simplified. It would then be necessary only to provide for integration of individual character by guiding children through a series of experiences graduated as to opportunity for acting in accordance with standards, and of graduated temptation to diverge from them.

This book completes one of the most notable researches in character education of recent times. The foundation is laid for a science of moral education, and well standardized tests are provided for further research. Probably no one, in this time of chaos of traditional and reasoned moral ideals and practices, could have performed the task attempted better than the authors have done, but when our mixed codes of morals have been revised in the development of a science of ethics, there will probably need to be a complete revision of all these tests of moral character.

Leominster, Mass. E. A. KIRKPATRICK

Formen des Gemeinschaftslebens jugendlicher Mädchen: Sozialpsychologische Untersuchungen in einem Fürsorgeerziehungsheim. By GERTRUD HEERMANN. Hamburger Untersuchungen zur Jugend- und Sozialpsychologie, No. 2. Leipzig, J. A. Barth, 1929. Pp. viii, 160.

This monograph describes the social structure that developed in an institution of 55 asocial young women, ranging in age from 14 to 20 years. The most inclusive social unit is that based on rank or influence in the group; rivalry for position and power ranges the members into a series of zones ranging from the most to the least effective. Within this totality minor social units appear: associations of two or more friends, the clique, groups for some purpose, groups stimulated by outside leaders, and others as they are commonly described by the social psychologist. These groupings, as well as the social behavior of the individuals, are interpreted as the normal expression of the persons in the particular environment that is provided. The characterization of each individual gives, in the main, the essential facts for tracing the resulting behavior and might well be read by those who are giving us large masses of undigested and often meaningless facts on problem children.

Students of social psychology will want to use this investigation to see the operation of the principles of social behavior in a group form not described in earlier literature. With other studies that examine carefully and minutely a small section of life, it will tend to lend precision to the large generalizations that constitute the substance of many treatises on the subject. To profit by the study it

will not be necessary to accept the presuppositions of the writer. Stern's view is apparent throughout. There is an unnecessary stress on examining the individual as a whole, and refraining from analysis and the investigation of aspects of the person. In fact, the discussion frequently bases conclusions on views that can come only from highly specialized experimental enquiry. For example, the statements on the respective nature of two individuals who will become friends in a given situation give the occasion for an experiment on an aspect of behavior rather than undifferentiated observation. The relation of drives and motives to the behavior studied can hardly be regarded as fundamental. We shall not advance our insight into human life by basing interpretations on the generalization that there is a struggle for existence in adolescent years or that "the longing for the understanding and like-minded person . . . . can be regarded as the leading drive in social formation."

Florida State College for Women P. F. FINNER

The Psychology of Religious Awakening. By ELMER T. CLARK. New York, Macmillan Co., 1929. Pp. 170.

Much as those questionary studies of conversion, made thirty years ago, have been condemned for their crudity of research technique, attempts to improve upon them have been rare indeed. That they should be early repeated was apparently believed by some at least of those who made them. With the passing of the years reasons for their repetition have accumulated. Lehman and Witty have shown the great change in collecting habits over the same interval, and the question has been properly raised if there may not have been like changes in the frequency and modal age of religious conversions. Clark's re-study here presented is therefore most welcome.

The method used was again that of the questionary. The questions show some effort to profit by the experience of those who made the early studies, but they manifest little influence of the extensive development of questioning technique which has come in the last decade. Returns were available from 2174 people, mostly students in small colleges. The data are excellently presented both in tables and in graphic forms. None of them seem to have been omitted. They are all there for future reference and comparison, a feature which will appeal strongly to those who have tried to do much with the earlier studies. The age curves verify that indication of a drop in the modal age of religious awakening already brought out by the Indiana Survey. But they do more. The returns are classified into types—definite crisis, emotional stimulus, and gradual. By tabulating the returns according to each of these forms separately the drop in the modal age is revealed to be characteristic of the last two only; the modal age for the crisis variety of religious conversion remains about where it was when Starbuck and Leuba and Coe and others made their studies. That there is a conspicuous decline in the frequency of the crisis variety is not surprising, but its definite determination is valuable. The relationship of different kinds of religious training, especially the conservative versus the modernistic, to the different forms of conversion is carefully worked out and given in detail. The treatment in general is cautious and convincing. Much as the statistically minded might wish for a

more elaborate treatment of the data, or for data collected in a manner permitting a more elaborate treatment, the reviewer is inclined to the opinion that this book must be accepted as the most important contribution to our knowledge of the conversion phenomenon that has appeared in a generation at least.

Psychologists who use the book will soon discover that the author is a clergyman rather than a psychologist. He speaks the language of religion more easily than that of psychology, and this leads to the usage of such phrases as "the sense of joy, peace, and happiness" and "the sense of increased responsibility" which are likely to irritate the trained psychologist. Perhaps if he had been a psychologist first of all the author might have sought to discover relationships to mental ability and academic achievement, to measures of social adjustment and fair-mindedness, or to measures of emotional stability (topics which the book will make every psychologist wonder about); but then again, had he been a psychologist he might not have done the job at all.

University of Oregon EDMUND S. CONKLIN

Psychologie der Waarneming. Een Studie over Gezichtsbedrog. F. J. T. RUTTEN. Nijmegen, N. V. Dekker & Van de Vegten, J. W. Van Leeuwen, 1929. Pp. 154.

This monograph was presented as a doctor's thesis to the University of Utrecht. The classical Müller-Lyer illusion was chosen for experimental exploration with a view also to the formulation of a general theory of perception. A rather complete account of the types or variety of figures used in earlier investigations (sketches of which from pages 6, 8, and 9 may well be copied for demonstration material) is rendered in the introductory chapters together with a critical historical review of earlier studies and theories. The technique of the experimental set-up is described in minute detail. It involved two lanterns focused toward the middle of a white screen 1 meter square in a dark room; the exposure-time was controlled by motor and rotating cut-out disks which allowed variation in two ways. Great care was taken to consider and to rule out all disturbing or variable factors, an endeavor being made to keep brightness, and size of figure, duration of fixation, and distance, under perfect control. The two original figures of the Müller-Lyer illusion were used in turn with length of accessory lines in 2/5 relation of the main line and with angles of the accessory lines of 30, 45, and 90°. (Three other variants of the figure were also tried.) The size of the figure could be varied from 4.5–100 cm. The distance of *O*, who was seated between the lantern and the fixation-point on screen, along the middle axis, varied from 20 to 350 cm. A table gives angles of vision from 2 to 78°. Twelve different exposure-times from 10 to 1000 sigma were tried. The figures were projected both horizontally and vertically in varying order of sequence of main and accessory lines. Two trained and seven untrained *Os* served during the experiments. The general duration of one sitting was one hour, with rest periods intervening. The instructions were as follows: "Lines will be projected in front of you; you will see them together for an instant. Tell me then if you perceive a complete figure and describe it, *i.e.*, how everything appears, behaves, and disappears." These instructions appear highly suggestive to the reviewer, and they may even partly explain the results obtained as due in no mean measure to the influence of the *Aufgabe*, bearing in mind especially the 7 untrained *Os*. It might be that if the instruc-

tions had been: "Something will appear in the visual field; you are to describe your experience in detail," the results would have conformed more to the *Gestalt* theory of perception or would have been near to those effected on the basis of Bentley's theory of organization. The study may, in this regard, serve to illustrate the all-importance for the formulation of any theory of perception of the attitude and the *Einstellung* instilled in the *O*. This crucial point has hitherto hardly been given its due attention. Under the conditions described above the author finds four phases of the formation of the figures: At the exposure of the figure and its parts, movements similar to those discussed by the Gestalt school were observed. It seems, however, somewhat arbitrary and dangerous to divide these movements into the two classes, (1) phenomena which are the consequence of the tachistoscopic exposure of an object, and (2) form-phenomena which must be considered as a result of the tendency to formation of a new whole from collective data, *i.e.*, as the *Durchstructurierung* of the observed complex. There is, in point of fact, no possible criterion for such a discrimination. The author's explanation of the Müller-Lyer illusion, as well as his theory of perception, finally ebbs out in an organization of 'meanings.' With its careful technique, its almost complete history of the illusion, the study will take its permanent place in this classical field.

Wittenberg College M. L. REYMERT

The Growth of Reason. By FRANK LORIMER. New York, Harcourt Brace & Co., 1929. Pp. xii, 231.

The sub-title of this book is: A study of the Rôle of Verbal Activity in the Growth of the Structure of the Human Mind. The volume makes a distinct contribution toward placing the psychology of language and of reasoning upon a scientific basis. Scientific 'approaches' are offered to studies in symbolism, in reasoning and in social psychology. Fortunately the author knows biology, psychology, and philosophy, and apparently enjoys the art of keeping an even keel while steering for his goal. New and difficult terms are defined with unusual clearness. The book abounds with new insights and new points of view without being radical or strained. The mind-body conundrum, the symbol-meaning bugbear, language and higher thought processes are among the problems admirably handled.

The contents appear by chapters and are expressed in analytic form, and the bibliography, also arranged by chapters, is placed at the end of the text.

Skidmore College LINUS KLINE

Principles of Experimental Psychology. By H. PIÉRON. Translated by J. B. Miner. New York, Harcourt Brace & Co., 1929. Pp. viii, 190.

The latest French edition of Piéron's book appeared in 1928, and was reviewed in this JOURNAL (41, 1929, 124). We now have it translated into English by Miner. Thus it is available for general student use as a text or for reference. Miner's translation is both adequate and pleasing. The sense of the French text has been rendered with utmost accuracy into English which retains the style of the original without an 'accent.'

Hobart College FOREST LEE DIMMICK

The Growing Boy. By PAUL HANLEY FURFEY. New York, Macmillan Co., 1930. Pp. viii, 192.

In *The Growing Boy* Furfey presents a very simple and readable account of the social characteristics and play interests of boys between the ages of six and sixteen years. Chapters I, II, III, and VIII deal with growth in general, the concept of developmental age, development during the first six years, and adolescence. The remaining six chapters are given up chiefly to a series of brief case studies of the play behavior of boys of different ages. Throughout the book, the fact that social development is a continuous process which is not marked off by any very sudden or discrete steps is emphasized. Developmental age, which is the writer's term for "the progressively increasing maturity of behavior which shows itself in the child's changing interests and in his whole behavior" shows an age-overlapping which is quite similar to that found for stature or mental age.

The value of the book is much enhanced by the well-selected list of references at the end of each chapter. It should form a useful addition to the library of the elementary teacher or the lay reader who is interested in child development.

Institute of Child Welfare FLORENCE L. GOODENOUGH
University of Minnesota

A Syllabus Work-Book for a Two-Level Plan of Instruction in Elementary Psychology. By H. H. REMMERS and G. C. BRANDENBURG. New York, Macmillan Co., 1930. Pp. 110.

This work-book contains a series of assignments in Gates' *Elementary Psychology* and Dashiell's *Fundamentals of Objective Psychology*, with supplementary questions and readings from other authors. There is also a list of exercises or more specific questions to be answered in writing. For the more able or more ambitious students optional assignments are suggested in the way of reports on additional readings, observations or exercises. These are more general in nature.

While the work is an attempt to take care of the more able students in the same classes with the less able, the method may not prove the most advantageous one, however, as there is little recognition that the able student knows much of the work at the start and may actually need a more advanced course rather than additional assignments beyond the usual ones.

University of Minnesota M. J. VAN WAGENEN

Sleep: Why We Need It and How to Get It. By DONALD A. LAIRD and CHARLES G. MULLER. New York, John Day, 1930. Pp. xi, 214, 13 charts, 2 tables.

This is an interesting and amusing book, popularly and carelessly written in semi-journalistic style, and resplendent with anecdotes. It is apparently of little scientific value since experiments and procedures are not described so that they could be repeated, and exact data are omitted. It has an appendix giving a brief 'Description of Typical Experiments' and a bibliography of 81 titles which are not referred to. There is no index.

University of Minnesota C. R. GARVEY